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THE PSYCHOLOGICAL REVIEW

DISCRIMINATION OF CUES IN MAZES: A RESOLUTION OF THE "PLACE-VS.-RESPONSE" QUESTION[1]

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Whether rats in mazes learn turning responses or places is a question which has often been subjected to experimental test. Data from different experiments conflict, and attempts at a definitive answer seem only to add to the confusion. This paper will defend the thesis that the place-vs.-response question is wrongly formulated, and that the data which fail to decide between place and response learning give an unequivocal answer to a question properly stated.

The place-vs.-response question has been approached largely through the use of the T maze, rotated in its visual surround. A typical arrangement showing runs reinforced in "place" and "response" learning is shown in Fig. 1, along with the fixed-maze problem in which both place and response may be learned. In both place learning and response learning, the maze is rotated on alternate trials at random. The place learner is always to go to the same place in the room, responding consistently to extra-maze cues but making different turns on different trials. The response learner makes the same turn on all trials, going to different places. One supposed test of whether place or response learning is more dominant is to compare rates of learning on these two problems. Another test is to train animals with the maze in a fixed position (place + response learning), and then rotate the maze for a test trial. The animal can now either make the same turn he has learned or go to the same place he has been going to, but not both. In this *direct opposition* experiment, the relative number of animals taking each choice is a test of the relative dominance of place and response.

In 1946, Tolman, Ritchie, and Kalish (23) proposed that in such a situation place learning is more natural and primitive for the rat than response learning, and place will dominate response in all tests. Their experiment supported the hypothesis, but some later repetitions have found response dominating place, or have found no difference.

An answer to these apparent contradictions is here sought by assuming that maze running depends on a multiplicity of cues, and that the rat learns differential responses to relevant cues in a maze just as he would in a discrimination box. A theory based on learning in the discrimination box will be applied to the maze situation. Since the discrimination theory to be used (15) is very close to earlier theories of the effect of sensory input on learning in mazes, theories which antedate the place-vs.-re-

[1] Dr. Richard L. Solomon suggested and drafted part of this paper (see footnote 3). His guidance and help are gratefully acknowledged.

[2] Now at Michigan State University.

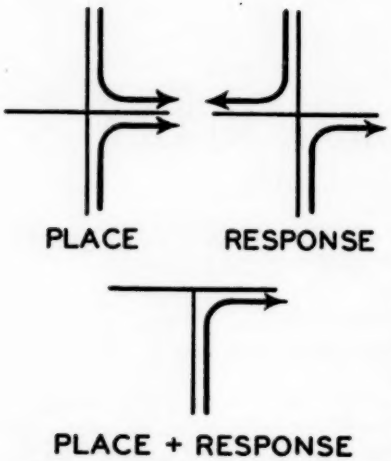

FIG. 1. Runs reinforced in place, response, and place + response (fixed maze) learning. Only one T maze is present on a given trial.

sponse controversy, it is useful to place the recent studies within their historical context and analyze all the data at once.

MULTIPLE CUE THEORY

Hunter (9, 10) and Honzik (8), among others, have proposed that learning and performance in mazes depends upon many cues, in all available modalities and from all sources. Depending on what stimuli are available to the rat, maze learning may depend on visual, auditory, olfactory, tactual, and kinesthetic stimuli. Visual, auditory, and olfactory stimuli may arise from within the maze or outside. The cues used by rats may be not only the specific physical stimuli but also patterns or arrangements of stimuli, so that changes which preserve the arrangement may not disrupt performance.

When a portion of the cues are removed by surgery or screening, or made irrelevant by interchanging maze units, rotating the maze, etc., learning of the maze is retarded, but mazes are often perfectly solved in the presence of known irrelevant cues.

The earlier formulation of multiple cue theory is somewhat refined by considering more recent theory derived from discrimination learning (15). This newer theory distinguishes between relevant cues, which bear a regular relationship to the correct path, and irrelevant cues which are not consistently related to the correct response.

A cue with a constant or predictable relationship to the true path will in the course of learning become "conditioned" to the correct response. All such cues will eventually be learned, and all will play a part in performance. If a certain cue is irrelevant, bearing a changing and unpredictable relationship to the correct response, it cannot be the basis of learning and is only a distractor. Such cues will become "adapted" during learning, and eventually will play no part in performance.

The rate of learning depends directly on the proportion of relevant, usable cues in the total set available.

When learning has been based on a variety of relevant cues and then some of these cues are scrambled and made irrelevant, the amount of disturbance reflects the relative importance of the newly changed cues. Though irrelevant cues will eventually be adapted, making important cues irrelevant reduces the proportion of relevant cues and thus retards learning. Disruption due to scrambling of learned relevant cues is attributed to the fact that such cues are not adapted, having previously been relevant. Recovery from the disturbance results from progressive adaptation of the scrambled cues.

If the learning program involves constantly introducing new cues which have not been present before, these new cues will always disturb performance, since they cannot have been attached to the correct response and they cannot have been adapted.

In an ordinary multiple-unit maze, the untrained rat responds erratically early in training, producing different kinesthetic stimuli on different trials. As a result, such stimuli are not consistently related to the true path, and alone they cannot be the basis of rapid learning. As performance based on other cues improves, the animal makes more nearly the same movement each trial, producing for himself a regular pattern of kinesthetic stimuli which can serve as partial basis for the learned habit. Highly skilled performance thus depends to some degree on kinesthesis, though such cues do not alone mediate learning to a significant degree.

The above application of the theory of discrimination learning to mazes does not differ in important respects from the assumptions and conclusions of Hunter and Honzik. The main refinement is in attributing the rate of learning to the *proportion* of relevant cues. The earlier writers do not make just this statement, though their statements may be interpreted to this effect.

No attempt will be made in this paper to apply the specific quantitative formulation of discrimination learning to multiple-unit maze learning. The effects peculiar to the *serial* character of maze learning are not reflected in discrimination theory. When a single-unit T maze is used, as in the recent place-response experiments, discrimination theory should apply exactly and quantitative predictions should be correct.

The multiple cue theory can be compared with the results of studies using multiple-unit mazes, with the understanding that specific quantitative values (error scores, trials to learn, etc.) depend both on the discrimination of cues and on serial patterns. An understanding of these earlier studies of maze cues is essential in gaining a clear insight into the place-vs.-response controversy.

THE HISTORICAL CONTEXT [3]

Studies of the sensory control of maze behavior of rats have been reviewed in detail by Munn (**14**), so only a broad summary will be given here, along with discussions of the main points.

Early studies of the role of sensory processes in maze behavior were motivated by the early conclusion by Small (1901) and by Carr and Watson (1908) that in complex mazes rats form kinesthetically controlled habits which become "automatic." This hypothesis is that kinesthetic cues, arising from one response in the maze, serve as the main cues to the next response in the sequence. Each response is associated with the previous one, and other sensory input becomes unimportant in maintaining performance. This conclusion was based on the erroneous assumption that if one sort of cue is important, others must be unimportant. The technique was essentially one of eliminating a single sense modality at a time, in each case observing that performance remains essentially intact. The only modality not disturbed was kinesthesis, which was assumed to be "the" crucial one. Hunter (**11**) has shown the flaw in this line of argument.

The "kinesthetic" hypothesis inspired a series of experiments designed to prove or disprove it. These experiments uncovered a wealth of information about the cues which in fact control behavior in various mazes. Fundamentally, the data were analyzed to determine the relative effectiveness of (*a*) kinesthetic stimuli, (*b*) intra-maze stimuli, such as visual, olfactory, and tactual stimuli from the maze itself, and (*c*) extra-maze stimuli, such as visual or auditory

[3] The bearing of these studies on the place-vs.-response question was pointed out to the author by Dr. R. L. Solomon. This section and the beginning of the next section are based in part on a personal communication from him.

stimuli from the room containing the maze. Intra- and extra-maze stimuli were broken down into components due to different sense modalities, again with the intention of evaluating each.

The methods used in assessing the role of sensory events in maze behavior were as follows: (*a*) surgical interference with receptor organs and neural pathways; (*b*) elimination of stimuli from the intra-maze or extra-maze environment; (*c*) introduction of distinctive stimuli to the environment; and (*d*) the controlled rearrangement of intra-maze and extra-maze stimuli.

In general, the results of these experiments were as follows:

(1) Simple alternation mazes can be learned by the rat on the basis of kinesthetic cues alone, but more complex mazes cannot be learned in any reasonable number of trials without the aid of visual, olfactory, or auditory cues. Removal of kinesthetic cues by surgical means does not greatly affect maze performance if intra- or extra-maze cues are left intact. However, once a maze performance is perfected on the basis of intra- or extra-maze cues, removal of such cues does not destroy the performance completely. As one would expect from the multiple cue theory, kinesthetic cues are relevant and conditioned to the correct response only when a regular relationship between the last response and the next correct response is established. Such a relationship exists in a simple alternation maze early enough for learning to take place. In more complex mazes, usable kinesthetic cues exist only after the maze is learned on some other basis. Since during learning kinesthetic cues are mostly irrelevant anyway, their removal does not retard learning.

(2) Mazes can ordinarily be learned on the basis of intra-maze cues alone, even if extra-maze cues are made irrelevant by rotating the maze in the room. Such rotation retards learning, however, especially if the maze is elevated and the room contains conspicuous visual cues. If the maze is kept in place in the room and its spatial arrangement is preserved while units of the maze are interchanged to make intra-maze stimuli irrelevant, rats can still solve the problem. Again, interchanging of stimuli retards learning. In many mazes, it should be noted, all blinds have a common visual appearance, being shorter than true alleys and ending within sight of the rat at the choice point. Interchanging units does not make such stimuli irrelevant. But even if the spatial pattern of the maze is scrambled, if the goal is in a fixed position relative to the extra-maze environment the rat can learn to run to the goal box without following alleys which lead away from the food. Thus, intra-maze and extra-maze cues are each separately capable of sustaining learning and performance in the maze. With one type scrambled, perfect performance can often be attained based on the other type, indicating that irrelevant cues are eventually disregarded. Retardation of learning due to removal or scrambling of important cues is consistently observed.

(3) In elevated mazes, where most of the visual field arises from outside the maze, extra-maze cues are usually more important than intra-maze cues. If the maze is enclosed in a homogeneous room, however, extra-maze cues are relegated to a minor role. Intra-maze cues are generally more important in alley or tunnel mazes, where the rat has at best an obstructed view of the outside. In a unidirectional maze, extra-maze cues are more important than they are in a maze which requires the rat to run in many different directions. The results support the idea that either type of cue may be the more important, depending on the relative amount of relevant stimulation stemming from each source.

(4) In general, two ways of assessing

the importance of a certain cue give comparable estimates. One method is to scramble the cue during learning, assessing its importance by the relative retardation in the rate of learning. The other method is to have the rat learn the maze with the cue relevant, then scramble it. The amount of disruption of the perfected habit indicates the importance of the cue. To an approximation, these two methods rank various classes of cues the same way. Removing a type of cue by depriving the animal of necessary receptors, as by blinding or deafening the animal or rendering it anosmic by surgical interference, is not the same thing as scrambling cues experimentally. Blinding, for example, removes both relevant and irrelevant visual cues, whereas rotating the maze makes otherwise relevant extra-maze cues irrelevant, and does not affect intra-maze visual cues. Thus, exact comparisons between the effects of scrambling and the effects of surgery cannot usually be made.

These findings suggest that kinesthetic cues are unimportant in the learning of complex mazes, but that both intra- and extra-maze cues are important. The relative importance of various types of cues depends on the maze and its surround, for rats seem to use various cues proportionally as those cues are available and relevant. Rats seem capable of overcoming the distraction of irrelevant cues.

The results are consistent with the conception that maze learning and performance depend on multiple cues, and that such cues are discriminated and responses learned to them in accord with the theory of discrimination learning.

"PLACE" AND "RESPONSE" IN SINGLE-UNIT T MAZES

In the light of the earlier extensive studies of the sensory basis of maze running, studies of place and response in T mazes appear as comparisons of extra-maze and kinesthetic cues. The Tolman-Ritchie-Kalish hypothesis that rats learn "places" rather than "responses" means, in the earlier terminology, that extra-maze cues are more important than kinesthetic cues. Since in earlier studies kinesthetic cues were found barely sufficient to permit any learning at all, whereas extra-maze visual cues bulk large in importance when they are available, it would appear that the dominance of "place" learning was indubitable, and the Tolman-Ritchie-Kalish experiment redundant.

It should be noted that Tolman's "place" formulation is inferior to earlier formulations. Tolman does not specify what cues are thought to make up "place" indications, and he does not identify "response" learning as learning based solely on kinesthetic cues. Though intra-maze cues do not seem to be "response" cues in Tolman's sense, it is not clear whether they are "place" cues or not.

Though the hypothesis that place learning will dominate response learning seems to follow in a general way from Tolman's concept of cognitive maps, his position was not really controversial because stimulus-response theorists did not believe response learning would be easier than place learning. The early Carr-Watson hypothesis, that maze habits are mainly controlled by kinesthesis, was by 1946 buried under conflicting evidence, and S-R theories stated that the stimulus components of the maze habits were quite likely to be extra-maze visual cues, especially if the maze is elevated and in a room full of such cues.

From these considerations, one should expect that the dominance of place learning is a foregone conclusion. The existence of a "controversy" is itself a

surprise. In order to show the nature of this controversy it is necessary first to consider how place-response experiments have been conducted and what results have been obtained. Since this recent literature has not received a definitive review, a relatively thorough analysis is presented here.

Tests of Place-vs.-Response Dominance

We first consider whether place learning in fact dominates response learning in the single-unit maze. Ten studies have compared the rates of place and response learning in single-unit T mazes or slight modifications thereof. Of these, seven found place learning faster (1, 3, 5, 22, 23, 24, 25), and two found response learning faster (7, 20), while one found that either could be faster depending on the intertrial interval (21). In direct opposition tests (rotation of the maze after the habit is learned on a fixed maze), one study showed that either place or response could be superior depending on the shape of the approach stem (18), another showed that either could be dominant depending on differential cues (27), and two others found response tendencies overriding place tendencies (2, 12). A modification of the opposition test consists of setting up conditions in which the animal alternates. Several studies have asked whether an animal alternates response or stimulus (place) characteristics (4, 6, 13, 26), and the answer has always been that place alternation is stronger than response alternation. These studies used enclosed alley mazes, "place" cues being especially distinctive intra-maze cues.

In summary, if we merely count titles, the impression is received that place tendencies are usually learned faster than responses, and that they are sometimes stronger in opposition tests, especially those depending on alternation. One certainly cannot draw the conclusion that place learning is always dominant, though, for response tendencies dominate in at least some conditions of seven different studies.

Conditions Associated With Place and Response Dominance

Since neither place nor response is uniformly dominant, we may consider the experimental conditions which make one or the other stronger.

Several writers (1, 7, 21, 25) have suggested that the relative dominance of place depends on the amount of differential visual stimulation. This is, of course, the position taken by earlier writers on general maze learning, though place-vs.-response studies are not usually analyzed in terms of this variable. Counting heads in the experimental reports, we see that the use of a homogeneous visual surround (a dome or enclosure, usually made of muslin, which prevents the animal from discerning any uncontrolled stimuli from the room about him) greatly predisposes rats to learn responses instead of places. All four experiments using such domes report response dominance (2, 7, 20, 21), though one (21) showed that response dominance could be neutralized by massing trials. The only other cases of response dominance reported (12, 18) were in plain rooms under low illumination, which may be thought of as approximations to dome-type enclosures. All other studies showing place dominance were done in open rooms (1, 3, 5, 22, 23, 24, 25) or in alley mazes with strong differential cues (6, 13, 26). One study showed that when the two places contrast sharply in illumination, place dominates, whereas when the illumination is more nearly equal, response tendencies dominate.

In summary, place tendencies dominate when visual stimuli at the two ends

of the maze are very *unlike,* and response tendencies dominate when such stimuli are relatively *alike.* In every case the domination seems to be quantitative—both place and response tendencies exist, but one is stronger than the other depending on stimulus conditions.

By inspecting the experimental reports we can get some idea of what constitutes a strong place cue for the rat. The most dramatic place dominance was found in two studies (17, 23) in which rat cages were located to one side, nearer one goal than the other. In one of these studies (17) it was found that the rats would not give up responding to one place in the room, despite a number of controls, until the cages were moved, following which almost all the rats reversed. Stimuli such as lights had only a slight effect on performance, compared with the rat cages.

When rat cages are placed directly behind the starting point and are thus not available as place cues, or when the room is devoid of cages, such visual stimuli as windows (in daylight studies) give rise to strong place preferences (1, 3), as do well-lighted rooms with many small objects in them (5, 25). Somewhat less striking but still consistent place dominance was shown in two studies in which one wall of a plain room (the wall behind one goal), was moderately well illuminated, the other wall not being illuminated at all (22, 24). The power of room cues can be estimated by noting the quick learning of a successive discrimination between rooms (19). Attempts to give differential cues inside domes by illuminating a 10-inch disc behind one goal with a 7.5-watt lamp (2, 12) or using 7.5-watt lamps themselves as cues, with (7) or without (20) overhead illumination, did not lead to place dominance. Symmetrical overhead illumination in an empty room produced response dominance with spaced practice (21). A small lamp on the floor below one goal, pointed to throw long shadows on the floor, gave slightly more place than response tendency (18). Ratio of illuminations rather than difference seems to be a crucial variable (27).

In summary, rat cages are very strong place stimuli, windows or objects in a well illuminated room rank next in power, and fairly strong differential lighting of walls of a plain room is also effective. Discs lit by 7.5-watt lamps, or such lamps themselves, are not usually enough to make place dominant over response in an otherwise homogeneous setting.

Some other variables are associated to some degree with place and response dominance. For example, all experiments using pigmented rats, (4, 22, 23, 24) show place dominance, except for one (12) which showed cases of both place and slight response dominance under low illumination. But white rats also show place dominance under good illumination (1, 3, 5, 25), so we may reason that it just happened that experimenters who intended to use strong visual cues also sometimes used pigmented rats to take advantage of their superior vision. The pigmentation of the rat cannot be shown to have any other effect on place and response dominance.

The noncorrection procedure (picking up the rat after an error) seems to lead to better response learning than the correction procedure (allowing the rat to find the food by retracing after an error). Using the place-response studies, the effects of correction cannot be isolated from the visual cue effects mentioned above.

One study (21) indicates that response learning is slowed by massing trials. Little more about massing effects in these studies is known, but most

of the studies employ at least moderate trial spacing.

There seems to be no interesting relation between place-vs.-response dominance and either the ages of rats used or the size of the maze employed. The slight tendency for smaller mazes to go with response dominance is more than explained by the tendency of experimenters using domes to use slightly smaller mazes for convenience.

Evaluation of the Place-vs.-Response Controversy

The details of the single-unit T maze experiments quite clearly indicate that there is nothing in the nature of a rat which makes it a "place" or "response" learner. The main factor determining the outcome of place-vs.-response experiments is the amount of extra-maze visual stimulation which differentiates the region around and behind one goal from the region around and behind the other. Such visual cues are relevant in place learning and irrelevant in response learning.

It seems reasonable to conclude that the place-vs.-response controversy, which seemed ill formulated when compared above with earlier maze studies, gives a distorted and confusing interpretation of the experiments designed to settle it.

It should be remarked that in single-unit mazes, kinesthetic cues appear sufficient to support quite rapid learning. This conflicts with Honzik's conclusion that "when all other avenues of stimulation are destroyed, kinaesthesis is helpless" (8, p. 56). Honzik's statement, if taken as applying generally to all mazes including single-unit ones, would be incorrect. The importance of a sense modality depends on the richness and relevance of stimuli in that modality which exist in the maze situation. In single-unit mazes, relevant kinesthetic stimuli apparently abound. In Honzik's 14-unit maze, animals do not make regular enough runs to give themselves a constant set of kinesthetic stimuli on which to build accurate performance. One may also consider Hunter's point that in complex mazes, with left and right turns required in irregular order, simple kinesthetic cues are irrelevant. Attempts to state the relative importance of sense modalities or of intra- and extra-maze cues in general, for all mazes, are akin to the "place-vs.-response" hypothesis in that they fail to incorporate the most important variable, the stimulus situation presented to the animal. Such attempts are, accordingly, doomed to failure.

PREDICTION OF T-MAZE DATA BY THE QUANTITATIVE THEORY OF DISCRIMINATION LEARNING

The single-unit T maze, being devoid of the serial characteristics of more complex mazes, may be thought of as a kind of discrimination-learning apparatus. When the T maze is rotated on random trials as in place-response experiments, extra-maze (place) and kinesthetic (response) cues are uncorrelated.[4] In place learning (see Fig. 1) place cues are relevant, and all others including response cues are irrelevant. In response learning, response cues are relevant and all others including place cues are irrelevant. In place + response learning, with a fixed maze, both place and response cues are relevant.

We may entertain the hypothesis of cue-additivity: that the set of cues rele-

[4] If the whole maze is rotated as a unit, intra-maze cues are relevant in response learning and are confounded with kinesthetic cues. If the starting stem is moved but the cross-arm is left in place, intra-maze cues are relevant in place learning, and are confounded with extra-maze cues. We shall assume that intra-maze cues combine additively with the cues they are correlated with, and shall not specify what happens to them.

vant in place + response learning is simply the sum of the place and response cues. This hypothesis can be tested quantitatively, using a theory of discrimination learning (15). In this theory, the rate of learning is set equal to the proportion of relevant cues, and the single number is called θ. The learning curve (or statistics such as total errors to mastery, total errors in n trials, trials-to-criterion, etc.) may be used to estimate θ: and, conversely, given θ the learning curve or any of its statistics can be computed in advance of experimentation. The equations involved and the methods of computation will not be repeated here.

Galanter and Shaw (5) used three groups of rats trained in the same apparatus and surround, under conditions of place learning, response learning, and fixed-maze or place + response learning, respectively. Using median trials-to-criterion [5] as an index of typical performance, the proportion of relevant cues in each problem was computed, using the equations of discrimination-learning theory. Theoretically, the proportion of relevant cues in the place + response problem should be the sum of the proportions in the other problems: $\theta_{P+R} = \theta_P + \theta_R$. The results of the computations to check this hypothesis are shown in Table 1. Place + response performance is predicted using only data from the place and response groups. Inspection of Table 1 indicates that the prediction is relatively accurate, although, since subjects in the place + response group ranged in trials-to-criterion from 0 to 3, the prediction is at the edge of the obtained distribution of scores.[6]

TABLE 1

MEDIAN TRIALS TO 10-OUT-OF-10 CRITERION AND PROPORTION OF RELEVANT CUES (θ) IN THE GALANTER-SHAW EXPERIMENT

Relevant Cues	Observed		Predicted	
	Trials	θ	Trials	θ
Place	5.0	.34	—	—
Response	33.5	.10	—	—
Place + Response	2.0	.58	3.1	.44

Scharlock (20) used a maze in a dome with either one light behind one goal or lights behind both. If there was one light it was the only source of differential place cues, so if there were two lights there were no place cues. Scharlock ran place, response, and place + response groups with one light. He also had a control group which made no progress on place learning with both lights—an expected result confirming that with a light behind each goal there were no place cues. One other group, which we may call "response-minus-place," learned a response with both lights on, and thus with no place cues available. Here again we can predict that the place + response group will yield a learning rate, θ_{P+R}, which is the sum of the rates of the place and response groups, $\theta_P + \theta_R$. In addition, the learning rate of the response-minus-place group should be faster than that of the response group, because of the

[5] Since retracing and "false-start" responses were counted as errors, animals had a higher probability of making an error than a correct response at the beginning of training. This bias in favor of errors has relatively little effect on trials-to-criterion, which is accordingly used as an index of performance. The parameter θ is estimated from trials-to-criterion by an approximation to the maximum likelihood method.

[6] Dr. Galanter, in a personal communication, noted that the place + response group was run after the other parts of the experiment were completed. In Galanter's opinion, the experimenters were by this time somewhat more skillful, and the place + response group had an advantage. This might account for the discrepancy between prediction and observation.

TABLE 2

ERRORS IN 28 TRIALS AND PROPORTION OF RELEVANT CUES (θ) IN THE SCHARLOCK EXPERIMENT

Relevant Cues	Observed		Predicted	
	Errors	θ	Errors	θ
Place	5.53	.216*	—	—
Response	3.84	.296	—	—
Place learning, no place cues (2 lights)	17.50	.000	—	—
Place + Response	2.28	.445	2.00	.512
Response − Place: no place cues (2 lights)	2.84	.366	2.66	.378

* Because of initial biases, the probability of correct response on the first trial is estimated at about .375 for all groups. The correction is made in the fashion shown in (16).

elimination of irrelevant place cues. Since θ_P is the proportion of differential place cues, it follows theoretically that $\theta_{R\text{-}P} = \theta_R/(1 - \theta_P)$. The results of computations to check these hypotheses are shown in Table 2. Inspection of Table 2 shows that the predictions are quite accurate. The discrepancies between predictions and observations are not statistically significant.

A third experiment by Blodgett, McCutchan, and Mathews (3) separates location and direction cues. The rat may approach the same location (for instance, the center of the room) from either of two directions if the maze is shifted appropriately. Location and direction are usually lumped as place cues, but in this experiment they are separated. Seven groups constitute the experiment, with one group learning each problem possible: location, direction, response, all combinations of two relevant, and the combination of all three relevant (fixed maze learning).

The data were reported in terms of "cycles," pairs of trials, which contained at least one error. From this it is not possible to make good estimates of θ, but an effort has been made to attain fair approximations. The estimates, and the corresponding predictions made by adding θ-values of problems with fewer relevant cues, are shown in Table 3. The results seem quite encouraging, under the circumstances.

SUMMARY AND CONCLUSIONS

Consideration of early studies of the sensory basis of maze learning, and review of place-vs.-response experiments, indicate that:

(1) There is nothing in the nature of a rat which makes it a "place" learner, or a "response" learner. A rat in a maze will use all relevant cues, and the importance of any class of cues depends on the amount of relevant stimulation provided as well as the sensory capacities of the animal. In place-response experiments, the importance of place cues depends on the amount of differential extra-maze stimulation.

(2) A multiple-cue theory of maze learning is successful in comprehending the major results of experiments using complex mazes, and the detailed results of place-response experiments using single-unit T mazes.

(3) Useful refinements of classical multiple-cue theory were taken from discrimination-learning theory. These are that irrelevant cues are adapted during learning, and that the rate of

TABLE 3

PROPORTION OF RELEVANT CUES (θ) ESTIMATED AND PREDICTED IN THE BLODGETT, MCCUTCHAN, AND MATHEWS EXPERIMENT

Relevant Cues	Estimated θ	Predicted θ
Location	.02	—
Direction	.11	—
Response	.08	—
Location + Direction	.13	.13
Location + Response	.10	.10
Direction + Response	.13	.18
Location + Direction + Response	.18	.21

learning depends on the *proportion* of relevant cues.

(4) Quantitative analysis of the results of certain place-response experiments indicates that place and response cues combine additively in the place + response (fixed maze) problem.

The writer's general conclusion is that further "definitive" studies of the place-vs.-response controversy, to prove that rats are by nature either place or response learners, would be fruitless since the issue is incorrectly drawn. However, use of the T maze to analyze the stimuli in maze learning holds promise of yielding a consistent quantitative account of how rats find their way. Such studies can build on the earlier work on more complex mazes.

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LATENT EXTINCTION AND THE FRACTIONAL ANTICIPATORY RESPONSE MECHANISM[1]

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I

Most students of learning would agree that in order for an organism to acquire an adaptive behavior pattern not only must certain appropriate response be learned, but other inappropriate or nonadaptive ones must be extinguished.[2] It is not surprising, therefore, that the attempt to provide an explanation of the manner in which response extinction occurs is at present a strategic area of concern for current theories of learning. Of importance to such theories is a procedure, employed recently in several studies (11, 32, 36, 38), that was designed to demonstrate that the strength of an instrumental response can be weakened prior to its first nonrewarded performance. This procedure involves placing experimental animals (rats), following the termination of training, directly into a now empty but previously baited goal box for a period judged sufficient to "inform" them of its present nonrewarding character. The effect upon response strength was observed when the subsequent performance of these *S*s in approaching the empty goal location was compared with that of control animals which had not received this pre-extinction experience. The procedure whereby an animal is introduced directly into an unbaited goal location after having acquired a response instrumental in securing a reward object previously contained in that location will be referred to as "latent extinction." While some investigators have questioned the effectiveness of this procedure (9, 37), others (10, 11, 23, 29, 32, 38) have demonstrated conclusively, in several different learning situations, that latent extinction does produce a decrement in the strength of the response to be eliminated. The fact that latent extinction has proven effective in this respect has been considered embarrassing to the S-R reinforcement theorist, since the implication appears to be that ". . . all that seems necessary for extinction is for the organism to *perceive* the absence of reinforcement" (12, p. 62). It is the purpose of the present paper to attempt to interpret the effects of latent extinction in a manner consistent with an S-R theoretical approach and, on the basis of this interpretation, to suggest hypotheses for further empirical study. It should be emphasized that the term "latent extinction" will be used throughout to denote a particular experimental procedure and *not* a process or a state of the organism.

Latent extinction has been employed primarily in two essentially different situations. The situation we will hereafter refer to as Type I was designed to measure the effect of latent extinction on the strength of a *previously established* instrumental response. The Type I situation involves training an animal to traverse a straight alley or acquire a position habit in a maze in order to ob-

[1] The writer wishes to acknowledge with appreciation the helpful comments of his colleagues, Professors Wayne Dennis, Elizabeth Fehrer, and David Raab.

[2] Extinction will be defined as the reduction in response strength that occurs following nonreinforcement. The mechanism mediating this reduction is left open, since in the opinion of the present author much more data are required before a complete and empirically adequate explanation of the extinction process can be formulated.

tain a reward object. Following training, the animal is placed directly into the goal location in the absence of the reward object for a predetermined period (latent extinction) after which "regular" response extinction is administered. The effect of latent extinction in reducing the strength of the instrumental response is indexed by the number of trials required to reach an extinction criterion. An experiment reported recently by Deese (11) serves to illustrate the Type I procedure. Deese trained rats to a position habit in a U maze with food as the reward. Following the termination of training, the experimental *S*s were placed in the now empty goal location for four 1-min. periods. When these *S*s were subsequently run to extinction, they yielded a significantly smaller proportion of correct responses than control *S*s that had not received the latent extinction experience. Deese's results indicate that a response *can* be weakened by placing *S* in the goal location without the presence of the reward object and that therefore, in at least some learning situations, performance is *not* a necessary condition for extinction.

The procedure that we will hereafter refer to as Type II is concerned with the effect of latent extinction on the capacity of secondary reward stimuli to mediate the learning of a *new response* in the absence of primary reinforcement. The training employed in the Type II situation involves the use of differential reinforcement in a straight alley. By means of this technique, the cues in the baited goal box come to acquire secondary reward value while those of an unbaited goal box of a different color do not. Following the termination of training, the animal is given latent extinction which, as in the Type I situation, consists of introducing him directly into the positive goal box in the absence of the reward object. The two goal boxes are then placed on a T maze and the number of choices to the side of the maze containing the positive goal box is taken as an index of the effect of latent extinction on the capacity of secondary reward stimuli to mediate new learning. The results of a recent experiment by Moltz and Maddi (33), to be discussed in detail below, attest to the efficacy of latent extinction in reducing the acquired reward value of previously neutral stimuli.

II

An expectancy theory of extinction (44) encounters little difficulty in providing an interpretation of the effects of latent extinction, since it places no special requirement on the response to be eliminated. All that appears necessary to decrease an animal's readiness to respond in accordance with an established expectancy is to change the relationship between environmental objects, and to allow the animal to experience the new relationship on one or more occasions. Since the acquisition of a new cognitive pattern only requires that *S*, when motivated (45), experience stimulus events in spatial and temporal contiguity, response performance as such is not made a necessary component of the extinction process. Thus placing *S* directly into the goal location, and permitting him to observe that the reward object is no longer present, should weaken his previously acquired cognition concerning the character of the goal location. As a consequence, he will show a decreased tendency to respond in accordance with this cognition.

Hull's inhibition theory of extinction (21), on the other hand, encounters a great deal of difficulty in providing an explanation of the effects of latent extinction (especially when latent extinction is employed within the framework of a Type I situation), since the performance of the response to be elimi-

nated is considered a necessary component of the extinction process. Reactive inhibition (IR) and conditioned inhibition (S^IR), which are assumed to produce extinction by jointly opposing reaction potential (S^ER), develop only in the course of response performance. This implies that either the response to be eliminated or some other response from which extinction effects can generalize must be performed before extinction can take place. Since the latent extinction procedure does not require that an animal perform the response to be eliminated, it could not produce an increase in the inhibitory factors that oppose reaction potential, and consequently could have no effect upon response strength. But while it is clear that Hull's response-induced inhibition theory of extinction requires the performance of the response to be eliminated, and is thus not adequate to account for the effects of latent extinction, Hull's general behavior theory makes no such requirement. In a series of early papers (**15**, **16**, **17**, **18**, **19**, **20**), Hull explored the possibility of applying a number of explanatory constructs to complex behavioral relationships in which reasoning, insight, knowledge, and purpose appeared to be operative. One of these constructs—the fractional anticipatory goal response—was used subsequently by others to mediate a wide range of behavior phenomena. For example, Kendler's analysis of switching behavior (**25**, **26**, **27**) and Osgood's mediation hypothesis (**35**) both rely heavily on the explanatory capacity of the fractional anticipatory response mechanism. Following the leads contained in Hull's early theoretical article, Spence (**40**, **41**, **43**) has provided a somewhat more rigorous analysis of the role played by this mechanism in the acquisition of a T-maze choice response, and has used this analysis to integrate latent-learning data and data concerned with the acquisition of opposed spatial responses under the simultaneous presence of hunger and thirst. It is the opinion of the present author that the fractional anticipatory goal response possesses the deductive potential necessary to mediate also an interpretation of latent extinction data that is consistent with an S-R reinforcement approach.

Spence has suggested (**40**) that, as a function of differential reinforcement during training, the stimulus traces of the cues in the positive goal box and those in the alley leading to it become (classically) conditioned to the goal response (e.g., eating or drinking). Through generalization, cues at the entrance to the correct alley acquire the capacity to evoke that fractional component of the goal response (r_g) not in conflict with the overt acts of the behavior sequence. This component produces a characteristic proprioceptive stimulus (s_g) which in turn becomes conditioned to entering and continuing locomotion in the alley leading to the positive goal box. Through the acquisition of this associative connection, s_g becomes a component of the habit strength determining the instrumental response. It has also been suggested that "through the intensity of its trace which . . . increases with the increased strength of r_g as the latter becomes more strongly conditioned, s_g determines the non-associative factor K" (incentive motivation) (**40**, p. 273). Spence implies that the magnitude of this nonassociative component is an increasing monotonic function of the intensity of s_g. Since the latter, in turn, is assumed to covary positively with the strength of r_g, such variables as the amount of reinforcement and the delay of reinforcement (which presumably affect the strength of r_g) play a role in determining the value of K. If it is recalled that

$$S^ER = f\,(M \times S^HR),$$

where

$$M = f(K + V + D),$$

it can be seen that r_g and its characteristic proprioceptive cue determine the excitatory strength of the correct response by contributing a component to S^HR and also by determining the value of K.[3]

But while the fractional anticipatory goal response has been considered by some students of learning to be a construct that possesses a great deal of explanatory fertility, it has been considered by others as an *ad hoc* device designed to rescue the S-R theorist from embarrassing experimental data. Koch, for example, maintains that:

> Among the potentially most fruitful items in Hull's bequest to the future are the many ingenious "peripheral" mechanisms which were elaborated in the first instance, in his earlier theoretical articles. . . . It is likely that concepts of this order point to factors which must be ultimately taken into account by any theory of behavior . . . (**28**, pp. 164–165).

On the other hand, Meehl and MacCorquodale write that ". . . r_g at present . . . is as readily available a *deus ex machina* for nonexpectancy theorists as the concepts of 'attention,' 'emphasis,' or 'perceptual threshold' are for Tolman" (**31**, p. 232). Since r_g appears to enjoy a somewhat ambiguous epistemological position, it would be well to clarify its meaning before attempting to provide an interpretation of the effects of latent extinction.

The use of a theoretical construct denoting a nonobservable response process carries with it the responsibility of providing a set of symptom relations by which the construct in question can be coordinated with observable events. A number of such reductions to data language is essential if the construct is to acquire univocal intersubjective reference, and if it is to be integrated with other constituents of the theoretical system in which it functions. One way in which a set of reduction symptoms can be provided for r_g is to conceive its relation to certain antecedent manipulable variables to be the same as that of any other molar response with which contemporary behavior theory has been concerned (e.g., bar pressing, spatial responses, salivation, etc.). This conceptualization implies that such variables as, for example, number of reinforced trials, quality and magnitude of the reward object, and hours of deprivation will exert the same effect on the strength of r_g as they have been found to exert on the strength of these molar responses. Once the relations postulated to hold between r_g and certain experimental parameters are clearly set forth it becomes possible to specify the effect upon r_g of variations in the value of any one of these parameters. However, it should be emphasized that, since r_g is an inferred response and as such does not possess a directly observable element, evidence relevant to variations in the strength of r_g can be obtained only by examining the overt behavior of the organism under certain sets of experimental conditions.

III

The Type I situation. Consider the situation in which an animal is required to turn right in a T maze to obtain a reward object relevant to an existent need state. When the excitatory tendency of the cues in the right alley to evoke a right turn becomes greater than that of the cues in the left alley to evoke a left turn, the animal will respond

[3] The additive relationship between V, K, and D presented here is consistent with Spence's latest formulation (**42**). Following Hull, Spence had earlier postulated a multiplicative relationship between these factors, the implication being, of course, that if any one of them is reduced to zero the excitatory strength of the instrumental response would also be reduced to zero.

above chance expectancy in the direction of the goal location. An important factor involved in the growth of the correct excitatory potential (S^ER_a) is the increase in habit strength (S^HR) resulting from each reinforcement. However, as the animal is repeatedly reinforced during the course of training, not only does S^ER_a approach its asymptote but, in addition, the overt consummatory response appropriate to the reward object (i.e., eating or drinking) becomes more strongly conditioned to the stimulus traces of the cues in the goal location. On the basis of the latter it appears reasonable to assume that when latent extinction is administered following the termination of training, that component of the consummatory response which can occur in the absence of the reward object (i.e., r_g) will be evoked by the cues to which its molar counterpart was previously conditioned. Since these cues are constantly present during latent extinction, it would be expected that r_g will be strongly and repeatedly evoked; each latent extinction "trial" providing the occasions for its *free repetition*.[4] Now if r_g is conceived as a response process that obeys the same behavioral laws as most molar systems, then it would follow that these successive nonreinforced emissions of r_g will produce a sharp reduction in its response strength with respect to cues in the goal location. If these cues are similar to those at the choice point, and if delay of reward during training was at a minimum, the extinction effects of r_g will generalize readily to antedating segments of the behavior sequence, so that at the start of the test trials r_g either will not be elicited at the choice point or will be elicited with greatly reduced excitatory potential. If latent extinction is effective in reducing the strength of r_g to the extent that it is subsequently not available at the choice point, then s_g, of course, will also not be available, and the associative connection established during training between s_g and the instrumental response will no longer contribute to the habit strength of that response. As a consequence of the withdrawal of this component of habit strength, the organism will manifest a decreased tendency to enter the correct alley during the test trials.

But assume that r_g was not completely extinguished during the latent extinction period, and that at the start of the test trials the cues at the choice point still possessed some excitatory tendency with respect to r_g. Even if latent extinction only reduced this tendency, a decrease in the strength of the established instrumental response would nevertheless be expected. It will be recalled that the incentive motivational factor K was postulated to be a nonassociative component of S^ER_a. Since the relationship between K and s_g is assumed to be monotonically increasing and, furthermore, since the intensity of s_g is a direct function of the r_g evoked, it would follow that any decrease in the strength of r_g will reduce the value of K. This reduction of K will, in turn, reduce the excitatory potential mediating the correct approach response, as a result of which the animal will show a decreased tendency to enter and continue locomotion in the alley leading to the previously positive goal box.

The Type II situation. The experimental procedures that we have labeled Type I and Type II are similar in that they both involve introducing an animal into an empty goal location after it has acquired a response instrumental in securing a reward object previously contained in that location. The difference

[4] Failure to conceive of latent extinction as providing the occasions for the *free repetition* of r_g led Gleitman, Nachmias, and Neisser (14) to dismiss the possibility of employing r_g to interpret the results of those experiments in which a Type I procedure was used.

between these procedures arises with respect to the responses with which they deal following the latent extinction period. It will be recalled that while the Type I procedure is concerned with the effect of latent extinction on the strength of an established response, the Type II procedure is concerned with the capacity of goal-box stimuli that have acquired secondary reward value to mediate the learning of a *new response* after latent extinction has been administered. It is to the latter procedure that we now turn our attention.

Consider the experimental situation in which an animal is trained to traverse a straight alley in order to obtain a reward object relevant to an existent need state. A differential reinforcement technique is employed by which, on a given number of trials, the animal runs to a white goal box that contains the reward object, while on the remaining trials he runs to an empty black goal box. By the principle of secondary reinforcement the cues in the white goal box will acquire reward value as a function of their association with the reinforcement situation. Hull (21, 22) suggested the fractional anticipatory goal response and its proprioceptive cue as providing the mechanism underlying the acquisition of secondary reward properties. The role that r_g plays in this respect develops by virtue of its intimate association with the reinforcement situation, this association establishing s_g as a secondary reinforcer. In order for any neutral stimulus to acquire reward value it must acquire the capacity to elicit r_g either by being conditioned to r_g directly, as in the case of stimuli that are spatially and temporally contiguous with the receipt of the reward object, or through generalization, as in the case of stimulus events that are similar to those actually conditioned. Hull further suggested that a stimulus that has acquired reward value will retain that value as long as it retains the power to evoke r_g. It would appear reasonable on this basis to assume the acquired reward capacity of a stimulus to be some positive function of the strength of the r_g to which it is conditioned, and for any subsequent reduction in this strength to decrease the reinforcing value of the stimulus.

Now with respect to the experimental situation being considered, assume that following training the positive and negative goal boxes are placed on opposite arms of a **T** maze, and that the animal is given a number of free trials in the *absence of the reward object*. When he first responds correctly and enters the positive goal box, r_g will be evoked by those stimuli to which it was conditioned during the course of training. As a result of the evocation of r_g, and the presence consequently of s_g with its reinforcing capacity, there will occur an increment in the excitatory potential of the correct response. As long as r_g continues to be evoked in the positive goal box, the correct response will be reinforced and will increase in strength relative to the incorrect response. But the excitatory potential of the correct response will not continue to increase unabated until some asymptotic value is reached. Since the reward object is not present during the maze series, the emission of r_g in the positive goal box following each correct choice will reduce its response strength, as a result of which the intensity of s_g and hence the magnitude of available secondary reinforcement will decrease progressively. When the stimuli in the positive goal box no longer elicit a threshold value of r_g, the excitatory potential of the correct response will begin to decrease, so that after several trials the animal will cease to respond correctly in excess of chance.

Now assume that latent extinction is administered at some time between the termination of straight-alley training and the beginning of the maze trials. For

reasons presented in connection with the Type I situation, placing an animal in a previously baited goal box provides the occasion for the free nonreinforced repetitions of r_g, each such repetition reducing its response strength. Because of the role that s_g plays as a reinforcing agent, it would be expected that, as the strength of r_g decreases during the course of latent extinction, the intensity of s_g, and hence the secondary reward value of goal-box stimuli, will also decrease. Depending on the duration of latent extinction and the presence of other conditions to be discussed below, r_g either will not be elicited in the positive goal box at the termination of the latent extinction period or it will be elicited with greatly reduced excitatory potential. In the event that r_g is completely extinguished with respect to goal-box stimuli, there will be no secondary reinforcement available to mediate subsequently the learning of a choice response in the maze. On the other hand, if the excitatory strength of r_g had not been eliminated but had been reduced to a near threshold value, then r_g will become completely extinguished *early* in the maze series. In any case, animals that have been given latent extinction would not be expected to show a strong tendency to choose the arm of the maze leading to the positive goal box, as compared with control animals that had not undergone the latent extinction experience.

By employing the fractional anticipatory response mechanism in a manner consistent with the requirements of the neobehavioristic system in which it was developed, we have been able to provide an interpretation of the effects of latent extinction in both the Type I and Type II situations. Within the theoretical framework employed it was necessary only to assume that r_g is repeatedly evoked during latent extinction, and that, as a consequence of this repeated evocation, r_g either becomes completely extinguished with respect to cues in the goal location or that a sharp reduction in its response strength occurs.

IV

The value of a theoretical construct should be measured not only in terms of how well propositions involving that construct explain available data but also in terms of the extent to which these propositions generate novel (i.e., not previously formulated) functional relationships. It must be determined, therefore, whether the fractional anticipatory response mechanism as employed in the present analysis is capable of mediating predictions concerning the influence on behavior of certain selected variations in both the Type I and Type II situations. Unless the present analysis possesses a prospective reference, propositions involving r_g have no value other than as vehicles in providing an economical description of already established empirical relationships. While such descriptions play an important role in science, the constructs involved function simply as analytic devices whose systematic significance is limited to furnishing summary statements of observed facts (5).

On the basis of our analysis of the effect of latent extinction on the strength of r_g we can specify the following general hypothesis: *Latent extinction will reduce the excitatory strength of an instrumental response to the extent that it reduces the exicitatory strength of* r_g. Following from this is the implication that if a variable is introduced, during latent extinction, that increases the degree to which r_g is weakened, a greater decrement in subsequent molar performance will occur than if latent extinction is administered in the absence of this variable. It will be recalled that r_g was conceived as a hypothetical response process whose relation to observable

antecedent events was assumed to follow the same behavioral laws as molar responses like bar pressing and salivation. This implies that all those experimental operations that have been demonstrated to reduce the strength of a molar response should affect similarly the strength of r_g. Since empirical evidence appears to indicate that reduction in the strength of these molar responses occurs as a monotonic increasing function of the number of nonreinforced evocations, it would follow that the more frequently r_g is evoked during latent extinction the greater will be the reduction in its response strength. On this basis we would be led to expect latent extinction to produce a pronounced decrement in subsequent molar performance under those experimental conditions that produce a rapid rate of emission of r_g during the latent extinction period. We turn now to consider some of these conditions.

Variations in the strength of the relevant drive. The results of several studies have indicated that the higher the level of a relevant (rewarded) drive during a series of extinction test trials the greater the rate of response evocation. For example, Jenkins and Daugherty (24) have shown that following training an increase in drive strength will result in the more frequent emission of a nonreinforced pecking response, while Skinner (39) has presented evidence confirming this relationship with respect to bar pressing. On the assumption that r_g obeys the same behavioral laws as these responses, we would expect a higher drive level during latent extinction to result in a greater increase in the number of nonreinforced evocations of r_g.[5] On this basis the following hypothesis can be specified with reference to the Type I situation: If after being trained to a position habit in a T maze under identical motivational conditions, one group of animals is given latent extinction under a high drive and another group under a low drive, the high-drive group will show a greater decrease in the tendency to choose the previously baited goal box when both groups are again run under the motivational level that prevailed during acquisition. A recent experiment by Pliskoff (36) is relevant to the present hypothesis. Animals under 8 hr. of food deprivation were trained in the performance of a T-maze turning response, after which latent extinction was administered under either 0, 8, or 22 hr. of hunger. When the extinction test trials were subsequently run under acquisition conditions it was found, in accord with expectation, that on the first test trial the 22-hr. group chose the previously correct side of the maze less often than the 0-hr. group. This result suggests a relationship between drive level during latent extinction and reduction in the strength of a previously established response.

The assumption concerning the relation between drive strength and rate of emission of r_g is also relevant to the Type II situation. It will be recalled that the acquired reward capacity of a neutral stimulus was assumed to be some positive function of the excitatory strength of the r_g to which that stimulus was conditioned. This implies that if increasing the drive level during latent extinction serves to increase the number of nonreinforced emissions of r_g (and thereby to decrease the strength of the latter with respect to goal location stimuli), then there should be a more pronounced reduction in secondary reward value the more intense the drive level under which latent extinction is administered. Evidence supporting such a hypothesis was reported recently by Moltz and Maddi (33). They employed

[5] The relation between drive strength and response evocation also follows readily from Hull's 1943 (21) and 1952 (22) postulate sets.

a differential reinforcement technique during training which consisted of running animals in a straight alley to a goal box of one color on rewarded trials, and to a goal box of a different color on nonrewarded trials. While all *S*s were under the same hunger drive schedule during training (22 hr.), they were under either a 0-, 22-, or 44-hr. drive at the beginning of the latent extinction period. The test trials were subsequently run in a T maze, with all *S*s again on the same hunger deprivation schedule imposed during training. It was found that the mean number of responses (in 15 free choices) to the side of the maze containing the positive goal box was 9.30, 7.10, and 6.30 for the 0-, 22-, and 44-hr. groups, respectively.[6] These results are in accord with theoretical expectation, since they indicate a relationship between motivational level during latent extinction and the capacity of goal location stimuli to mediate subsequently the learning of a new response.

Variations in the strength of an irrelevant drive. Hull has proposed that the drive strength available at any given amount is a function not only of the need for which the goal object in the situation is appropriate but also of all irrelevant (nonrewarded) drives operative at the time. It is the total need state, consisting of both relevant and irrelevant drives, that is assumed to combine with S^HR (in addition to the nonassociative factors V and K) to determine excitatory potential. In *Principles of Behavior* (21) Hull stated as a corollary that, with the number of reinforcements held constant, response strength will increase as some positive function of the magnitude of an irrelevant drive.

Webb (46) reported the results of a study designed to determine the role of an irrelevant drive (thirst) in activating a habit structure when the drive under which the habit was originally developed (hunger) was satiated. He found the strength of a panel-pushing response to be approximately an increasing linear function of the intensity of the irrelevant drive. Brandauer (6) subsequently confirmed this relationship, using a different operant response. Since we have assumed that r_g follows essentially the same behavioral laws as most molar responses, we appear to be in the somewhat uncomfortable position of having to propose that r_g appropriate to a relevant hunger drive will increase in strength in approximately linear fashion with increases in the magnitude of an irrelevant thirst drive. However, the implications of a theory or of a postulate are determined not only by the relations or laws specified but also by the conditions operative in the experimental situation to which these relations or laws are applied. As a case in point, consider the situation into which there is introduced an irrelevant need that provides a drive stimulus which elicits response *incompatible* with a previously established criterion response. Under such conditions it would not appear reasonable to expect the strength of the criterion response to increase as the irrelevant need becomes more intense. Indeed, only when conflicting habit tendencies are not activated would a criterion response, whether it be "molar" or "molecular," be expected to increase with increases in the intensity of an irrelevant drive stimulus.

The results of several studies (1, 2, 7, 13) in the area of secondary motivation provide evidence in support of this contention. For example, Amsel and Maltzman (3) have shown that, when

[6] The F ratio for the difference between these drive levels was significant well below .01. No significant difference in maze performance was found between control groups which were not given latent extinction but which were made differentially hungry during the latent extinction period.

anxiety is added to an existing motivational complex, the strength of a previously established consummatory (drinking) response will increase if the experimental situation is arranged so that responses conditioned to the irrelevant anxiety-drive stimulus (e.g., crouching, face washing, escape responses, etc.) cannot compete with the drinking response. On the other hand, if the situation is arranged so that response competition is permitted, the rate of drinking will be sharply reduced. These results, in addition to those provided by Kendler (26) concerning the "incompatibility" of fractional anticipatory eating and drinking responses, make it appear reasonable to assume that anticipatory drinking responses ($r^{g}d$), when instigated by an irrelevant thirst drive, will compete with anticipatory eating responses ($r^{g}e$), and consequently will decrease the rate of emission of the latter.[7] Following from this is the implication that after training under moderately intense hunger, the stronger the thirst drive that is subsequently introduced (thereby increasing the strength of $r^{g}d$) the greater the *reduction* in the rate of evocation of $r^{g}e$.

This analysis generates an important deduction with respect to both the Type I and Type II situations. Assume that an animal learns to approach a distinctive goal location under moderately intense hunger, and that as a function of this training the cues in the goal location acquire a strong excitatory tendency with respect to $r^{g}e$. If latent extinction is administered subsequently under both hunger and thirst, the presence of incompatible anticipatory drinking responses should *reduce* the number of nonreinforced evocations of $r^{g}e$; this reduction being greater the more intense the thirst drive. On this basis, and on the basis of the previously formulated assumption concerning relation between rate of emission of r_g and the influence of latent extinction on molar performance, we can state the following general hypothesis: If an organism is trained to acquire a response under moderately strong hunger but with ad libitum consumption of water, the effectiveness of latent extinction in reducing the strength of this response will *decrease* as the intensity of thirst during latent extinction increases. More specifically, in the Type I situation we would expect that if, after learning a position habit under 22 hr. of hunger, one group of animals is given latent extinction under the same hunger drive but in conjunction with 3 hr. of thirst, and another group in conjunction with 22 hr. of thirst, the latter will yield significantly *more* responses to extinction when the motivational complex of both groups is subsequently made identical with that which prevailed during acquisition.

Since the relation between intensity of an irrelevant thirst drive and the rate of emission of $r^{g}e$ would be expected to obtain in a Type II situation as well, we may also state the following hypothesis: After an approach response to food is established under moderately intense hunger, a greater reduction in the acquired reward value of goal location stimuli will occur if, in conjunction with the prevailing hunger drive, latent extinction is administered under 3 hr. of thirst as compared with 22 hr. of thirst.

[7] On the assumption that anticipatory drinking responses are in some manner "incompatible" with anticipatory eating responses, Kendler has attempted to explain what he has called the switching phenomenon, i.e., ". . . the tendency of rats after learning to go to one end of a T maze to obtain dry food to switch their spatial responses when made thirsty" (26, p. 179). He tested several deductions based on this assumption, one of which was that the conflict between $r_g e$ and $r_g d$ would be intensified (and hence the amount of switching increased) when S is shifted to a strong as compared with a weak thirst drive. The results obtained were consistent with this deduction.

Unfortunately there is no empirical evidence relevant either to this hypothesis or to the hypothesis formulated with reference to the Type I situation. Several experiments are, however, being conducted in the writer's laboratory in the attempt to determine the role of drive interaction during latent extinction.[8]

Reward immediately prior to latent extinction. In the presence of the appropriate drive state, a reward object with which an animal has had previous commerce will serve typically as an "unconditioned" stimulus for the consummatory response, in the sense that the latter will be regularly elicited when the reward object is presented. Since r_g has been conceived as a hypothetical process representing a fractional component of the consummatory response, it appears reasonable to assume that r_g possesses this functional characteristic in common with its molar counterpart. On this basis we would expect that, with the primary drive state remaining relatively constant, the frequency with which r_g is evoked will increase during a period following consumption of an appropriate goal object. However, if a large amount of reward is consumed, a reduction in the primary drive state would occur which, in turn, might offset or prevent entirely an increase in the rate of emission of r_g. On the other hand, a small amount of reward, while not appreciably modifying the drive state, might not be sufficient to effect this increase. Presumably there is an optimal amount of reward that would produce the desired result. With respect to food as the reward object, experimental evidence reported by Maltzman (30) indicates this amount to be within the range of *1000 to 2000 mg.* when the animal is under a hunger drive of approximately 22 hr.[9] On the basis of Maltzman's results, we would be led to expect that if, after training under hunger motivation, an animal is fed about 1500 mg. of food 20 sec. prior to latent extinction, an increase in the rate of emission of r_g will occur during the latent extinction period. This increase in rate of emission will, according to the present analysis, produce a marked reduction in the strength of r_g, as a consequence of which latent extinction should be more effective in reducing subsequent molar performance. Thus in the Type I situation it would be expected that if latent extinction is preceded by the ingestion of a reward object appropriate to the prevailing drive state, a greater decrement in the strength of the established instrumental response will occur, while in the case of the Type II situation a greater decrement in the acquired reward value of goal location stimuli would be expected.

The hypotheses that have been presented in the present paper have been concerned with the relation between certain experimental variables introduced during latent extinction and the rate of emission of r_g. Further experimental work and theoretical analysis might also be directed toward determining whether variations in molar performance following latent extinction can be ascribed to other parameters of r_g function. In addition, the effect of latent extinction might also be investigated with regard to the influence of such training parameters as the quality and magnitude

[8] These studies are being carried out in collaboration with Miss Nina Tokarow and Mr. Leonard Rosenblum.

[9] Maltzman found that hungry rats fed about 1200 mg. of food approximately 20 sec. before being run in a straight alley tended to run significantly faster than a control group that was not pre-fed. Several other investigators (4, 8, 34) have also reported that small amounts of preliminary reward will facilitate the performance of an instrumental response. Maltzman has indicated how these results can be interpreted in terms of the fractional anticipatory response mechanism.

of the reward object, number of reinforced trials, and the nature of contextual stimulus conditions.

V

The present paper represents an attempt to provide an interpretation of latent extinction that is consistent with an S-R reinforcement approach. The effects of latent extinction on the strength of an established instrumental response and on the secondary reward value of goal location stimuli were analyzed. It was found that propositions involving the fractional anticipatory goal response in conjunction with other propositions of neobehavioristic theory are adequate to account for these effects. Several hypotheses were suggested concerning the influence of certain variables on the rate of emission of the anticipatory goal response during the latent extinction period. Evidence relevant to these hypotheses was considered.

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THE CELL ASSEMBLY: MARK II[1]

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The purpose of this paper, as its title implies, is to suggest a revision of the neural construct that Hebb (5) has called a "cell assembly." The cell assembly plays a fundamental role in Hebb's behavior system, especially in his admirable treatment of perceptual learning, but the proposed changes are not intended to affect these psychological implications of the construct; they are aimed, rather, at making the underlying neural processes more explicit. As Hebb has mentioned (8), he tried to limit himself to using only known, or highly probable, properties of neurons in setting up his model; but it is clear that these soon proved to be inadequate, and he then had to introduce *ad hoc* mechanisms to explain how such artificially simplified elements could combine to produce the observed molar behavior. We can now see that from many standpoints the theory would have been better if the additional postulates had been introduced at the neural level, where they could have been more easily evaluated and tested; but on the other hand, such a theory might never have been given a hearing.

There is little doubt that flaws exist in the arguments Hebb uses to derive the molar properties of his construct. As will be shown below, if we try to operate with only those properties and connections of individual neurons that Hebb considered respectable, the behavior of the resulting network is incompatible with that required for the cell assembly. Some of the inconsistencies can be eliminated simply by taking into account neurophysiological discoveries made during the last ten years, but in order to derive a system having anything like the characteristics required for the cell assembly it will be necessary also to invoke a number of purely hypothetical neural processes.

As far as can be ascertained from *The Organization of Behavior* (5), only four factors are considered to determine whether a cortical neuron will fire: (*a*) the number of impulses bombarding the neuron from all sources during the few milliseconds in which temporal summation is assumed to take place; (*b*) the "strength" of the synapses concerned (the strength of a synapse may increase beyond its native value in accordance with the provisions of a learning postulate); (*c*) whether the neuron is refractory; and (*d*) by implication at least, the effect of neural fatigue. In the network that Hebb postulated as a model of the cortex, neurons with the abovementioned properties are randomly interconnected, each neuron having efferent and afferent connections with many others. Some of the neurons can be fired by sensory input.

Such a network is somewhat analogous to the nuclear fission reactor. In that machine a neutron may either induce a fission, in which case it releases several new neutrons, or it may be absorbed or lost without releasing any other neutrons. If the neutron flux in the machine is to remain constant, all the neutrons released at a fission must

[1] The writer wishes to acknowledge the influence on this paper of many helpful arguments with his colleagues at McGill, especially those with Drs. James Olds and Seth Sharpless, and with Professor D. O. Hebb. The paper was written while the author was receiving support from the Foundations' Fund for Research in Psychiatry, and from the Ford Foundation.

be lost except one. For stable operation, in other words, the multiplication factor (the average number of fissions produced by the neutrons released at a previous fission) must be one. Likewise, in the cortical network, a fired neuron may fire several others, or it may send impulses to refractory, fatigued, or inadequately facilitated neurons and produce no further firing. If the average neuron fires one other neuron, the total activity in the network will remain constant.

The analogy is not quite complete, however; in the fission reactor the multiplication factor is slightly reduced by an increase in the neutron flux, but in the cortical network just the opposite is true, the multiplication factor increasing with total neural activity. This results from the effect of summation at synapses. When a neuron is firing by itself, the probability of its firing another is small; but when many neurons are firing at the same time, they provide a facilitatory background for each other, and one of them may then be able to fire several neurons to which it sends efferents. Thus, the number of neurons fired by an average neuron will increase in proportion to the total number of neurons firing, a state of affairs which precludes stable operation. In the nuclear reactor, if the multiplication factor should fluctuate to a value of 1.001, a few more neutrons will be released than are lost, and the total flux will increase; at this higher flux the multiplication factor will be reduced again to one. If the same fluctuation should occur in the cortical network, a few more impulses will circulate, the general level of facilitation will rise, and the multiplication factor will increase still further. This increase will only be halted when all the neurons in the system are firing as fast as their refractory periods will allow. Unlike the nuclear pile, therefore, the cortical network has only two levels of stable operation, "full on" and "full off." Neither of these states fulfills the requirements of Hebb's cell assembly. It is hardly necessary to add that the phase sequence, which is essentially a chain of associated cell assemblies representing a "train of ideas," bears as tenuous a relationship to the basic neural axioms as does the cell assembly itself.

The fact that Hebb's constructs are not rigorously derived is not as serious for the over-all theory as it might seem, because many of the more molar physiological principles that Hebb introduces are valid and important for the understanding of psychological phenomena, irrespective of the exact neural mechanisms involved. Nevertheless, the possibility of bridging the gap between the physiological and the psychological levels with a more substantial structure presents a fascinating challenge, especially in view of the improvements made to the supports on the physiological side since *The Organization of Behavior* was written.

A New Physiological Interpretation of the Cell Assembly

In what follows it will be recognized that we are adopting Hebb's descriptions of the cell assembly and the phase sequence as far as their psychological properties are concerned, but instead of trying to reconcile these to a minimum of oversimplified neural data, further postulates will be introduced in an attempt to provide an explicit relationship between the constructs and their elements.

The first additional postulate concerns inhibition. At the time when Hebb was developing his theory, many physiologists were strongly opposed to the idea of neural inhibition, largely because it was difficult to fit into the electrical theory of synaptic transmission. There was a feeling that all inhibitory phe-

nomena could be explained by lack of facilitation, or by invoking the principle of refractoriness, and Hebb seems to have accepted this view. Since then, the work of Eccles (3) and others has provided a better understanding of synaptic transmission, and inhibitory transmission is now generally accepted.

The processes underlying learning have still not been revealed by the neurophysiologists, and to fill this lacuna there seems to be nothing against retaining the postulate suggested by Hebb: "When an axon of cell A is near enough to excite a cell B and repeatedly or persistently takes part in firing it, some growth process or metabolic change takes place in one or both cells such that A's efficiency, as one of the cells firing B, is increased" (5, p. 62).

Another assumption that we shall make here is that the cell assembly involves mainly cortical neurons. It is to be understood, however, that connections between cortical cells may often take place via cells in the thalamus, or other subcortical nuclei. In the model to be described, the fine-structure of the network has to be taken into account; Lorente de Nó's beautiful diagrams of the cortex (11) are invaluable for this purpose. They reveal a complex vertical organization amongst the different layers, including one conspicuous circuit to which Lorente himself draws attention. This is the return of collaterals from the long descending axons of some cells back to the region of the cell body, where they appear to end on Golgi type II, or similar short-axon cells. The Golgi cell axons proliferate in baskets around the bodies of the long-axon cells ". . . so that the discharge of the cell with a short axon results in powerful, practically simultaneous stimulation of a large number of other cells" (11, p. 303). It might appear from this description that the feedback circuit is admirably suited to produce uncontrolled

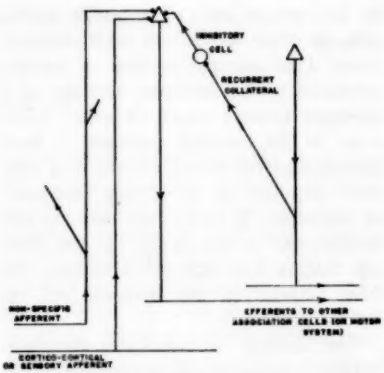

FIG. 1. Diagram of the postulated connections of cortical neurons.

epileptic discharge in the cortex, but recent microelectrode work by Eccles, Fatt, and Landgren (4) indicates that in the spinal cord, where a similar organization of long and short-axon cells is to be found, the short-axon cells inhibit the larger ones. It is likely that this is also true in the cortex.

The following highly simplified organization will therefore be assumed for the cortex (Fig. 1). Only two types of neuron are considered, those with long axons (having excitatory connections with other parts of the cortex or with the motor system), and those with short axons (which have local inhibitory connections). Those of the short-axon type receive impulses from the long-axon cells in their vicinity via recurrent collaterals, which leave the main shaft of the axon near the inner layer of the cortex and return toward the surface. Apart from those in the sensory cortex, which receive additional afferents from the sensory pathways, the long-axon cells receive their input from two major sources: (*a*) from other cortical cells, either directly via cortico-cortical association fibers, or indirectly by way of distributing cells in the association nuclei of the thalamus; and (*b*) from the nonspecific

projection system, which will here be regarded as a purely excitatory arousal system. This second source of afferent stimulation must play an important role in motivation, and it will be discussed in that connection later; for the moment, it need only be noted that we postulate that the facilitatory bombardment it provides is necessary for the conduction of impulses within the cortico-cortical network.

It appears from Lorente's diagrams (11, pp. 296, 298) that the long axons from the association cells branch and travel through the white matter to a number of different parts of the cortex. Each of these branches divides again as it ascends through the layers of the cortex to synapse with other long-axon cells. With this in mind, we can now proceed to analyze the activity in this network in the way that we previously analyzed that of Hebb's model. We shall start by assuming that no cortical cells are firing, but that there is a sufficient facilitation from the nonspecific projection system to make transmission between cortical cells possible. Suppose now that one cortical cell is somehow fired, and that it has effective connections to several other long-axon cortical cells (ten, let us say). These ten long-axon cells will in turn fire about a hundred others, and so on. At this rate the activity would soon snowball to an astronomical value, so that it clearly cannot continue to multiply for very long; in fact, the spread will quickly be checked by the firing of inhibitory cells. As the density of firing cells increases, impulses from many sources will arrive almost simultaneously in any small region of the cortex. When a long-axon cell is fired by one of the impulses, it will fire the short-axon cells in its vicinity and so prevent many subsequently arriving impulses from firing other neighboring cells. Thus, many of the impulses circulating in the cortex will be lost because they arrive in the regions of strong inhibition surrounding recently fired cells. Finally, a level of activity will be reached at which only ten per cent of the impulses are able to fire cells, so that in the example given (in which one cell could fire ten others in the absence of inhibitory influences) a state of equilibrium will be reached. If more long-axon cells start to fire, the regions of inhibition will be crowded together and less than one tenth of the circulating impulses will fire new cells; the activity must therefore drop back to the equilibrium value. If, on the other hand, too few cells are fired, the multiplication factor will increase above unity and the total activity will increase again. It is true that many factors have been neglected in this analysis; some of them are important and will be considered in a moment; other difficulties (such as the fact that when the level of neural activity rises, the increase in inhibition will be offset to an indeterminate extent by increased temporal and spatial summation) cannot be dealt with until better quantitative data are at hand. In the meantime we can only postulate that the parameters involved have values within the limits necessary to allow the network to behave in the manner described.

For reasons that will be clear later, it is desirable to construct the model in such a way that the equilibrium activity continues to circulate within a single group of cortical cells, instead of progressing continuously to new cells at each step in the firing. To this end, the following postulate will be made regarding the anatomical organization: the short-axon inhibitory cells which receive recurrent collaterals from a long-axon cell have fewer inhibitory connections to that particular long-axon cell than they do to other long-axon cells in the region. This is illustrated in an exaggerated way in Fig. 2, where it is seen

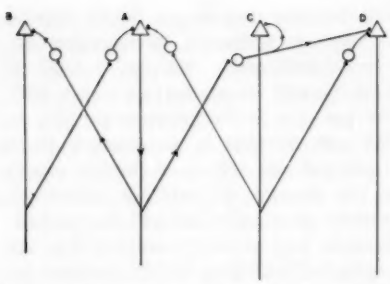

FIG. 2. Types of recurrent inhibitory connections.

that when Cell A is firing it causes the inhibition of its neighbors, B, C, and D, but is not itself inhibited. In fact, because the surrounding long-axon cells cannot now be fired, there is no way in which the short-axon cells discharging on to A can be fired. Therefore, as long as A continues to fire, it protects itself from being inhibited. This is not the only mechanism—perhaps not even the most likely mechanism—by which the desired result could be achieved (repetitive firing during the hypersensitive phase of the recovery cycle would also tend to confine the activity to the group of neurons that were already firing), but it is a useful hypothesis on which to base subsequent discussion.

Let us now see how this postulate will affect the equilibrium activity that can be sustained in our model of the cortex. There is a good chance that the cell which originated the activity will receive an afferent impulse from some other cell which is fired later in the avalanche. If it does, there will be nothing to prevent its firing again in response to the returning impulse, because its original discharge will have inhibited neighboring cells, thus greatly reducing the probability that it will be inhibited itself. When it fires again it will also be able to re-excite the ten cells that it fired previously, because they will have likewise remained free from inhibition. Thus the whole group will tend to re-excite itself, confining the activity to those neurons in which it first spread. If by some chance the original cell does not receive connections from any of the cells subsequently fired, some of the next ten cells, or those further along in the chain of activity, will do so, and the final result will be much the same.

It now remains to see what prevents the activity from persisting in this group of cells indefinitely. A property of most cells in the nervous system is that if they are subjected to a constant source of excitation their frequency of discharge steadily falls. We may therefore expect a decline in the intensity of firing in the group. As each cell adapts, it will deliver fewer impulses to other cells in the group, and reduced bombardment will combine with the adaptation to slow down the activity. On the other hand, there will be a concomitant decrease of inhibitory firing, so that the over-all activity will not decline as rapidly as it otherwise would. However, there is a limit to the compensation that can be provided in this way, because when the inhibition becomes too small some of the cells outside the group, which were previously prevented from firing, will be released. The firing of these cells will then bring in fresh short-axon cells which will immediately inhibit any fatigued cells in the vicinity. Thereupon, the balance of excitation and inhibition will be disturbed in regions to which the affected long-axon cells send impulses. If most of the cells in the group were already becoming adapted, or fatigued, the disturbance will snowball in reverse; each cell that stops firing causing connected cells to stop also. In this way the firing of one group might come to an abrupt end, and another group of cells take over. Note that although the activity will swing from one group of cells to another, it

can never die out; long before that happened the level of inhibition would become so low that all the unfatigued cells would be able to fire. The self-maintained cortical activity described above is, as the reader has no doubt realized, intended to form the basis of the revised version of the cell assembly.

One behavioral phenomenon which Hebb explains in terms of the cell assembly is "set." In order to do the same with the present model we shall have to add a further neural postulate. Perhaps the simplest type of behavior exemplifying "set" is that in which a stimulus, A, followed by another stimulus, B, gives rise to the response, C, although neither A nor B alone will produce the response. Presentation of A induces a set to respond to B. In neural terms, it seems that after a cell assembly has ceased to exert a direct control on an animal's behavior, it continues to influence the way in which that behavior develops, perhaps for a period of minutes. Only two categories of neural process seem able to account for this long-lasting aftereffect of a cell assembly; either some neurons of the group representing Stimulus A, for example, continue to fire, though the main body of the activity is now taking place in cells of the group representing Stimulus B (this is the "active" trace hypothesis), or the activity A leaves behind it physical or chemical changes which last long enough to influence subsequent firing patterns (the "latent" trace hypothesis). Hebb attempts to explain "set" in terms of the first of these alternatives—the active trace hypothesis—and the same process might be applicable to the present model. However, the second mechanism seems to offer much more interesting possibilities, which we shall now proceed to explore. The hypothesis might be formally stated as follows: If afferent impulses fall on a cortical cell but do not fire it (because of inhibition or lack of adequate background facilitation), they nevertheless leave the cell with a lowered threshold, the effect dying away with a time constant of many seconds. This influence of one cortical neuron on another will be called "priming." This postulate complicates the original model slightly because now, at the moments of transition from one pattern of cortical activity to another, we have to take account of the residual priming left by earlier activities, as well as of the momentary excitations. For example, a neuron that receives synaptic connections from neurons of several previously active groups will have a better chance of firing than a neighboring neuron that has been stimulated only by neurons of the most recently active group.

We must now examine the effect of sensory afferents on this postulated cortical system. The sensory projection areas of the cortex are regarded for this purpose as the final distribution centers for sensory impulses. The long-axon cells with specific-sensory afferent connections are assumed to have widespread efferent connections to the rest of the cortex, so that a change in the pattern of sensory input will result in multiple changes of the local fields of excitation there. If the cortex is already intensely active, and if the sensory change is a slight one, the added excitation will be unable to affect the ongoing pattern of cortical firing. The cells which are not already firing will be too strongly inhibited by those that are firing to be influenced by the input. Behaviorally, this means that the animal will ignore the stimulus and pursue its former course. On the other hand, if the stimulus is a strong one, or if the cortex is relatively inactive when it is presented, the sensory impulses will determine, or significantly influence, the pattern of the next activity in the cortex.

The learning mechanism can now be

made explicit. It has been postulated that if a neuron helps to fire another neuron on several occasions, it becomes a more potent agent for firing that neuron subsequently. This clearly implies that the neurons of a group that fire one another, in the way that has been described, will tend to bind themselves together more closely. Let us assume that one group has been activated on several occasions, and that consequently each neuron in the group has strengthened its synaptic connections with one or more other cells in the group, but not with any of the nonfiring cells to which its connections were initially equally strong. At this stage, if a neuron in the group is fired, the likelihood of its firing other cells of the group will have been increased; and the more cells of the group that are so aroused, the more the remaining members will be subjected to preferential excitation. This selective effect will often cause the total group activity to be established, starting from the firing of only a fraction of the constituent neurons. Moreover, each time this concerted firing takes place, the connections will be further strengthened, and the probability of the group's firing as a unit will become still greater. This process is equivalent to Hebb's mechanism for building up a cell assembly by repeated presentation of a "perceptual element"; its significance for the explanation of perceptual learning and concept formation has been discussed in some detail by Hebb (5, 6) and need not be repeated here. It must not be supposed, however, that after a stimulus has been presented often enough it will become able to determine the total pattern of the assembly; there will always be an inconstant "fringe" of cells that fire because of some previous cortical activity, metabolic conditions, or other factors independent of the stimulus. This fringe plays an important role in the association of one cell assembly with another, as we shall now see.

Associative learning was mentioned earlier as being the linking together of cell assemblies into phase sequences. This will usually occur because the assemblies in question have been aroused contiguously. It has been assumed up to now that the neurons that initiate an assembly activity are themselves fired by a sensory event, but clearly once a group of cells has strengthened its interconnections sufficiently, it does not matter which of its elements are fired first; the activity will in any case spread to the rest. If enough cells of one assembly are left in a primed or excited state by firing in other cell assemblies, that assembly may burst into activity when a previous one fades out. The question we have to answer is how such an interrelationship between two assemblies could be established by experience.

Let us suppose that two stimuli, giving rise to Cell Assemblies A and B, respectively, are presented in succession a number of times. Under these conditions some of the fringe neurons of Assembly B will be determined by A, and so will not be random any more. In other words, some of the cells primed by A will be active with Assembly B on every occasion that the stimulus pair is presented, and these cells will start to acquire stronger connections with the cells of the B assembly. Before long, in fact, they will become members of the assembly. When this has happened it will be possible for these cells, which on the first presentation of the stimulus pair were merely a part of the random fringe of B, to arouse the whole B cell assembly in the same way that any other fractional firing of the assembly would arouse it. It remains only to point out that these particular cells are the ones that are so strongly primed by the activity of the A assembly that they have a high probability of firing when

it stops (which is how they became part of the firing of B in the first place), and the method by which two assemblies become associated will be evident. Of the cells released from inhibition when Assembly A dies away, those that have on previous occasions fired with (and thus acquired strong connections with) cells of the B assembly, will be able to recruit the rest of the cells of that assembly. On the other hand, the successors of Assembly A which are incompatible with firing in the B assembly will have no strengthened connections, and will be promptly suppressed as the rest of the B assembly begins to fire.

It may be observed that the amount of perceptual "overlearning" that has gone into the development of the assembly, before its association with another, will make a great deal of difference to the ease with which the linking is achieved. If the total assembly action can spring from the firing of, say, five per cent of its constituent cells, links will have to be strengthened to a smaller number of cells than would be necessary if twenty per cent of the cells had to be fired to insure assembly action. That is, if the intragroup connections are weak to start with, the new cells will have to acquire stronger and more numerous connections to the rest of the group in order to compensate for the deficiency. Another point to be remembered is that only rarely will a single cell assembly be able to fire another one unaided, as in the example given. In most cases, the assembly aroused by association will have been primed by many previously active assemblies before it builds up enough sensitized cells to enable it to fire without its proper sensory input. This is an explanation of the importance of context, and set, for association; the arousal of a particular association may depend on the surroundings being similar to those in which the original learning took place, because the firing of a number of cell assemblies by the environmental stimuli is necessary to build up priming in the required group.

The model described above represents a brain mechanism capable of associating contiguously presented stimuli. This is only part of the apparatus needed to explain an animal's behavior in a learning situation, but it is an essential part. The most important other mechanism has to do with motivation. In this paper motivation will be considered to influence learned behavior in two ways: (*a*) by affecting the rate of learning, and (*b*) by helping to evoke responses.

In order to discuss motivation from a physiological standpoint, we must return to the system which was mentioned briefly before, the nonspecific projection system. It will be remembered that our model of the functional organization of the cortex was developed on the assumption of a fixed level of facilitatory bombardment from this system, such that one long-axon cortical cell was able to fire ten others in the absence of inhibition. If in the illustration we had assumed a lower level of nonspecific activity, the total cortical firing would have been less, though not proportionally so because of the accompanying decrease in inhibitory firing. It might be remarked, in passing, that a number of loose ends may be rather neatly tied together if it is assumed that impulses reaching a long-axon cell in the cortex from other cortical cells do not actually fire that cell, but induce a long-lasting depolarization that sensitizes it to bombarding impulses from the arousal system. Thus the cell would be *primed*, but not fired, by other cortical firing, and then fired by the next few impulses that it receives from the nonspecific arousal system. Such a mechanism would be consistent with the observation of Li and Jasper (9) that the rate of cortical firing increases with the level

of arousal; it would also be in agreement with the data of Clare and Bishop (2) on the long-lasting dendritic depolarization set up by association afferents. Moreover, the work of Bradley (1) might be interpreted as indicating that the arousal system afferents are cholinergic, and as acetylcholine is destroyed instantly on being released it is unlikely that the arousal system impulses would give rise to any long-lasting effect in the cortex.

Lindsley (10) has indicated that there is a rough correlation between the motivational and emotional states of an animal and the level of activity of its arousal system; and Sharpless (12) has shown that novel stimuli are more potent stimulators of the arousal system than are familiar ones. It is also generally true that a hungry or otherwise uncomfortable animal has an increased level of arousal activity, especially in the presence of a satisfier. It thus seems that conditions which increase firing in the nonspecific system are those which tend to promote learning. There is still dispute as to how essential reinforcement might be for learning, but there is little doubt that some sort of "emphasis" on the relevant stimuli accelerates the learning process. In terms of our model, this means that cortical cells are linked together more rapidly when the rate of cortical activity is higher—a most reasonable conclusion. It might also be deduced from the model that *some* association should be possible as long as *any* cortical cells are firing (during dreams, for example). The reinforcement controversy thus reduces to the question of *how much* firing is going on in the cell assemblies concerned, reinforcing agents serving to increase that firing through their effects on the nonspecific projection system. According to this theory, therefore, the arousing rather than the need-reducing properties of the reinforcement are those important for learning. Evolution would account for the fact that, for most present-day animals, need reducers and dangerous situations tend to be particularly arousing.

It should be noted that, in the examples given, it has not been a response but rather a relationship between stimuli that has been learned. It is not too difficult to extend the model to account also for the association between stimuli and responses, but we then have to face the problem of why the stimulus is sometimes followed by the response and at other times not. No doubt this is sometimes due to the failure of effective association between the cell assemblies concerned, but we have introspective evidence which suggests that we may know what to do to achieve a goal, but not be sufficiently interested to perform the actions. This brings us to the problem of the effect of motivation on performance. Unfortunately, we cannot do justice to this topic in the space of this paper, but a brief indication will be given as to how the model might be able to handle it. We must assume that many motor neurons will be fired along with the neurons of the cell assemblies we have described, but because they must compete with postural and other reflexes for control of the musculature, they can only produce a response if they fire at a high enough frequency. Below some threshold value, cortical firing has no effect on overt behavior. It is suggested that the arousal system is responsible for raising cortical activity above this threshold when the stimulus conditions are sufficiently exciting. (However, as Hebb has suggested [7], if the firing becomes too vigorous, some other factor may intervene to prevent further responses.) In the case of motivation set up by expectancy of reward or punishment, it must be assumed that the arousal system has been excited, not by the stimulus actually present, but by a cell assembly representing the moti-

vating stimulus which, through previous experience in the situation, had become associated with the cell assembly for the actual stimulus. The cell assembly for the motivating stimulus will be able to fire the arousal system because of an association built up through innumerable presentations of the motivating stimulus, under conditions where it fired both the cortical and the subcortical systems simultaneously. Thus the environmental stimuli fire their cell assemblies, these arouse the assembly of the motivating stimulus by association, and this in turn fires the arousal system, also by association.

An example might make the proposition clearer. When a rat is first placed in a maze, the arousal value of the novel surroundings will be sufficient to raise the level of cortical firing so that more or less randomly associated "motor" cell assemblies will influence overt behavior; the rat will explore. If no incentive is introduced, the effect of novelty will wear off and the rat will lie down and doze. However, if the rat is food-deprived, and food is placed in the maze, the interaction of the stimuli from the food with the cells in the rat's nervous system that are sensitized by deprivation will produce persistent excitation of the arousal system. Moreover, cell assemblies fired by the food stimuli (if the food is familiar) will have strong connections with the cells in the arousal system, so that even if the deprivation is not severe the presence of food will be likely to produce arousal. These cell assemblies for food stimuli will, of course, become associated with any other cell assemblies active at about the same time (those representing the sight of the food dish, or the feel of the maze floor, for example). Therefore, when on a subsequent occasion the hungry rat is returned to the maze, one of the assemblies fired by the stimuli in the starting box may be able to excite, by association, the assembly which represents the food, and thus, indirectly, the arousal system. The firing in the latter will not die out as soon as the cell assembly which aroused it ceases to fire; activity in the arousal system does not stop abruptly (possibly because its activity releases adrenalin into the blood stream). It will therefore continue to bombard the cortex at an increased rate for perhaps several minutes, and the motor component of any cell assemblies occurring during that time will fire intensely enough to produce a response. If, for example, a running response has been associated with the starting-box stimuli, the rat will actually run; if the same cell assembly (for running) had become active before the arousal system had been fired by the association between the starting-box stimuli and food, then the cortical firing would probably not have been intense enough to elicit overt movements. The same sluggishness would be expected if the animal was sated, so that the cells of the arousal system were not sensitive to food stimuli or their associated cell assemblies.

The above example is admittedly superficial. More complicated situations, such as avoidance learning, or extinction, cannot be explained without introducing still more postulates, in particular a system for inhibiting movements. The value of such a model would be primarily to demonstrate that it is possible to design a machine that would not only "want" or "avoid" arbitrarily designated stimuli, but also learn what to do about them under all environmental conditions to which it was sensitive. Whether the design would bear any relationship to the actual mechanism of animals which can also do these things could be known only after further research.

SUMMARY

A neural model has been presented, based on an "association-of-ideas" para-

digm of learning, similar to that used by Hebb. In it groups of neurons (cell assemblies) become representors of stimuli, and can then be linked together by being fired contiguously. The model differs from Hebb's in that an inhibitory regulatory system is postulated which limits (to a minute fraction of the total) the number of cortical neurons that can fire simultaneously, and insures that those firing are dispersed as widely as possible. A further change is introduced to meet the paradox that cell assemblies can be associated with one another without losing their individuality and being submerged in a composite new cell assembly. In association, it is not the cells of one assembly that acquire connections with the cells of another; instead, cells primed, or sensitized, by the first assembly become incorporated into the second. Thus, one principle of learning—the binding of cells into a group by repeated simultaneous firing—fulfills a double role; when the newly added cells are predominantly primed by sensory input perceptual learning results; and when the new cells are primed by the firing of another cell assembly, associative learning results.

Because the effect of priming lasts for many seconds, it is possible for a cell assembly to accumulate the sensitizations induced by the activities of a number of associated assemblies, and so increase the probability that it will itself fire.

The dual role of motivation—the facilitation of learning and the elicitation of responses—has been discussed in terms of the arousal effects of the nonspecific projection system on the postulated cortical network.

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LEVEL OF ASPIRATION AND DECISION MAKING[1]

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The purpose of this paper is to discuss two related topics: (*a*) the role of a person's level of aspiration in his decision making, and (*b*) the measurement of level of aspiration in a decision-theory context. The discussion of the second topic will include a brief summary of some experimental evidence which lends support to the methodological stand taken here.

The notion of level of aspiration is invoked in reference to the goal-striving behavior of an individual when he is presented with a task whose outcome can be measured on an achievement scale. Level of aspiration refers to the particular achievement goal for which the person strives.

The concept of level of aspiration was introduced by Dembo (8), and the first experiment in the area was conducted by Hoppe (14). It is a familiar concept to psychologists and others, having been the topic of extensive discussion and experimentation in the last quarter of a century. An early review of the literature is given by Frank (13). Rotter has offered a critical review of the methodological aspects of level of aspiration studies (19). An exceptionally important theoretical article, by Lewin, Dembo, Festinger, and Sears, appeared in 1944 (16).

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Decision Theory and Level of Aspiration Theory

It is the contention of the present writer that the psychological situation established in level of aspiration experiments may profitably be characterized as a *decision* situation, for from the alternative possible goals the individual must decide for which goal he will strive.

It is a remarkable fact that, by a simple change in nomenclature, the theoretical model used by Lewin *et al.* (16) in the prediction of the choices (decisions) of individuals in a goal-striving situation—a model based on the work of Escalona (11) and Festinger (12)—may be rendered fundamentally equivalent to the theoretical model employed by decision and game theorists. This latter theory, first advanced by Bernoulli (2), discussed by Ramsey (18), and formalized by Von Neumann and Morgenstern (28) and by Savage (20), states that under conditions of uncertainty individuals behave as if they were attempting to maximize expected utility. According to these and other decision theorists, an individual's decisions underlying his choices among alternatives involving uncertain outcomes (outcomes with stated probabilities of attainment) are based on the utilities of the entities (objects, actions, goals, etc.) and on the probabilities (subjective probabilities, for most decision theorists) associated with attainment of the entities. The decisions are a function of these two variables (utility and subjective probability) in that the individual seeks by his choices to maximize the sum of the products of probability and utility, i.e.,

he acts so as to maximize

$$SEU = \sum_i p_i u_i,$$

where *SEU* is subjectively expected utility (cf. [10]).

Equivalence of terms. Lewin *et al.* assert that the level of aspiration set by the person is a function of three factors: (*a*) "the seeking of success," (*b*) "the avoiding of failure," and (*c*) the "cognitive factor of a probability judgment" (16, p. 376). The last-mentioned factor refers to the individual's *subjective* probability of achieving a goal. (Lewin *et al.* use the term "subjective probability." See [16] p. 361 *et seq.*) Each of these three concepts of Lewinian theory may be "translated" directly to an equivalent concept in decision theory. The achievement scale may be thought of as a utility scale, where *utility* refers to the subjective value of an entity (goal, action, object) as distinguished from its objective or "face" value. It is the contention of this paper that each level on an achievement scale has a certain utility for a person, and that therefore level of aspiration may be conceived as a "point" on a scale of utility. An achievement which represents "success" in Lewinian terms may be thought of in decision theory terms as one which has "positive utility." An achievement which represents "failure" to the person may be thought of as having "negative utility" for him. And the Lewinian notion of "subjective probability" needs no translation, for the same term is used in decision theory.

The demonstration that certain concepts in Lewinian level of aspiration theory are equivalent to certain key concepts in decision theory is important because of the consequences it may have. Because of these equivalences, two important bodies of research literature may be synthesized, with benefit to both. The benefit to traditional psychologists, accustomed to dealing with terms like "success" and "failure," will accrue because of the much greater formality and rigor characteristic of decision theory, a rigor unknown to many psychologists. The benefit to decision theory will come because the synthesis will render available to decision theorists a large amount of existing experimental evidence with relevance to their work which has not yet been recognized by them.

Equivalence of formal statements of relations. The similarity between the two theories does not rest with equivalence of their variables. Also comparable are the formal statements of the relations among these variables.

For example, Lewin *et al.* (16, p. 360) state that:

> The individual faces the possibility of succeeding or failing and the positive or negative valence of such a future success or failure on the various levels is one of the basic elements for the decision. To determine the valence (*Va*) of each level (*n*) of activity [*Va* (A^n)] we have to consider the negative valence that future failure has on that level [*Va* (*Fai* A^n)] and the positive valence of success on that level [*Va* (*Suc* A^n)].
>
> $$Va\ (A^n) = Va\ (Suc\ A^n) + Va\ (Fai\ A^n).$$

When discussing the individual's felt probability of success (16, p. 362), they also state that:

> The most simple assumption is that this "weighted" valence of success [^{c}Va (*Suc* A^n)] is the product of the valence and the probability of success
>
> $$^{c}Va\ (Suc\ A^n) = Va\ (Suc\ A^n) \cdot Prob\ (Suc\ A^n).$$
>
> The corresponding formula for failure is:
>
> $$^{c}Va\ (Fai\ A^n) = Va\ (Fai\ A^n) \cdot Prob\ (Fai\ A^n).$$

If Lewin *et al.* had proceeded to the logical conclusion of these ideas, they could have stated that

$$Expected\ Va\ (A^n) = Prob\ (Suc\ A^n) \cdot Va\ (Suc\ A^n) + [(1 - Prob) \times (Suc\ A^n)][Va\ (Fai\ A^n)],$$

which in decision theory terms is given as

Expected Utility = *Probability* · *Utility of Success* + (*1* − *Probability*) · *Utility of Failure*

in which it is asserted that the individual chooses among alternatives (sometimes called "courses of action") so as to maximize expected utility.

This formula is equivalent to that given by Von Neumann and Morgenstern (28), and has been utilized in studies of decision behavior by Davidson, Siegel, and Suppes (7), Mosteller and Nogee (17), Siegel (21), Hurst and Siegel (15), Edwards (10), and others (see [9]).

In the discussion of Lewin *et al.*, the problem of obtaining a rigorous measurement of utility (degree of valence) and of subjective probability is virtually disregarded, despite the fact that these are the variables upon which the entire structure of the model depends. In the literature of decision theory, on the other hand, attention has been given to this important facet. See, for example, the experiments by Mosteller and Nogee (17) and by Davidson, Siegel, and Suppes (7) on the measurement of utility in an interval scale and on the measurement of subjective probability, and also those of Siegel (21) and Coombs and Komorita (6) on the measurement of utility on an ordered metric scale.

Another contrast between the two theories rests on the fact that in decision theory the formal theoretical models are based on a set of assertions from which consequences can be derived by logico-mathematical arguments. The assertions are referred to as axioms of the system (cf. [20]). If the model is used to interpret "real world" phenomena, it is possible to know, without ambiguity, what aspect of the model is analogous to what thing (or action) in the real world.

When such an abstract and formal system is available, it sometimes happens that the mathematical system will serve as a model for more than one situation in the real world. This fortuitous event may reveal analogies or relations of whose existence scientists were previously unaware. This seems to be the case in the present instance. A formal theoretical model has been constructed which is useful in interpreting decision behavior in general. It is contended in this paper that this model will encompass decision behavior in goal-striving or achievement situations, i.e., in situations where level of aspiration is an important variable of behavior, and furthermore that level of aspiration may usefully be defined in decision-theory terms and measured in a decision-theory context.

LEVEL OF ASPIRATION AS A DETERMINANT IN DECISION MAKING

Suppose an individual is given the task of finding as many as he can of five hidden objects, which will be his to keep. Suppose further that this individual aspires to find and thereby acquire at least four of these. That is, the acquisition of four or more of the objects is defined as "success" by this individual, and the acquisition of any fewer will constitute "failure" for him. Now suppose that before he attempts to find the objects, he is asked to choose between the following alternatives: (*a*) a 50–50 chance of getting either four of the objects or none of the objects, or (*b*) assurance of getting three of the objects. That is, suppose that the outcome of the task is completely determined by the individual's choosing either of the alternatives above and then by the toss of a "fair" coin. If the individual chooses Alternative (*a*) above, and if the coin lands "heads" he gets four objects, whereas if it lands "tails" he gets none at all. On the other hand,

if he chooses (b) he gets three objects if the coin lands at either heads or tails.

Now since the individual has set the acquisition of four objects as his level of aspiration, and the acquisition of any fewer will be considered as a "failure" by him, it seems clear that he will choose Alternative (a), despite the fact that objectively Alternative (b) has greater expected value. In other words, Alternative (b) may have greater expected *value*, but Alternative (a) has greater expected *utility* for this person. His level of aspiration has made the utility of the objects nonlinear with their objective value.

In game-theory terms, the above situation may be described as a one-person game in which the subject chooses between two alternatives, each of which is a probability-combination of two outcomes. The format for the game is:

	Alternative (a)	Alternative (b)
If heads occurs	you get 4	you get 3.
If tails occurs	you get 0	you get 3.

The subject chooses the column; the outcome of the toss of the coin determines the row.

Now if the subject has Level of Aspiration 4, and if therefore he chooses Alternative (a) over (b), this tells us that for this individual the difference in utility between 4 and 3 objects is greater than the difference between 3 and 0.

That conclusion is based on this reasoning: if

$$\text{Expected Utility of } (a) > \text{Expected Utility of } (b), \quad (1)$$

where $u(x)$ is read as "the utility of acquiring x objects" and interpreted to mean the subjective value of x, i.e., its worth to the person, then Statement 1 can be written

$$p \cdot u(4) + (1 - p) \cdot u(0) > p \cdot u(3) + (1 - p) \cdot u(3). \quad (2)$$

If p is understood to be subjective probability, and is known to be one-half, then Statement 2 may be written

$$u(4) + u(0) > u(3) + u(3) \quad (3)$$

and

$$u(4) - u(3) > u(3) - u(0). \quad (4)$$

That is, when Alternative (a) is preferred, we may infer that Statement 4 above is true, i.e., that the difference in utility between 4 and 3 objects is larger than the difference in utility between 3 and 0 (**21**, pp. 212–213).

We can see from the above that for this individual the acquisition of four or more objects has positive utility. The acquisition of three or fewer has negative utility. Zero utility is represented by some point between three and four. It should also be noticed that *the level of aspiration is associated with the largest difference on the utility scale.*

We may cite another example to illustrate the same points. Suppose a student is enrolled in a course in which he aspires for the grade of B. Before the examination on which the course grade is based, he is asked to choose between the following alternatives:

	Alternative (a)	Alternative (b)
If Event E occurs	you get an A	you get a B.
If Event Non-E occurs	you get a C	you get a B.

If the individual believes that the probability of the occurrence of Event E is .50, it seems clear that he will choose Alternative (b), since his level of aspiration is to achieve a grade of B. By this choice, the individual demonstrates to us that the difference in utility between B and C is greater than the difference in utility between A and B. That is, on his utility scale B is closer to A than to C. See Fig. 1.

FIG. 1.

Now the same person is asked to choose between these:

	Alternative (*a*)	Alternative (*b*)
If Event E occurs	you get a B	you get a C.
If Event Non-E occurs	you get a D	you get a C.

If his subjective probability toward Event E is .50, he will choose Alternative (*a*), for in terms of his level of aspiration any grade less than B is a "failure." His selection of Alternative (*a*) demonstrates that for him the difference in utility between Grades B and C is larger than that between C and D, as shown in Fig. 2. Again we see that the level of aspiration is associated with the upper bound of the largest distance on the utility scale.

LEVEL OF ASPIRATION DEFINED

Before we offer a formal definition of the level of aspiration, we may summarize what has been said above concerning it:

1. The level of aspiration of an individual is a position on his utility scale of an achievement variable.
2. The lower bound of that position is neutral in utility, i.e., all points below the lower bound have negative utility (psychological feeling of dissatisfaction) associated with them and all points above the lower bound have positive utility (psychological feeling of satisfaction) associated with them.
3. The level of aspiration is associated with the upper bound of the largest distance on the utility scale.

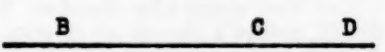

FIG. 2.

Each of the characteristics of level of aspiration enumerated above is incorporated in the following formal and more rigorous definition of level of aspiration:

The level of aspiration of an individual is a point in the positive region of his utility scale of an achievement variable; it is at the least upper bound of that chord (connecting two goals) which has maximum slope; i.e., the level of aspiration is associated with the higher of the two goals between which the rate of change of the utility function is a maximum.

It may be seen from this definition that the difference in utility between achieving at the level of aspiration or achieving at the next lower level is greater than is the difference in utility between achieving at the level of aspiration or achieving at the next higher level. In other words, the level of aspiration is that goal which has the

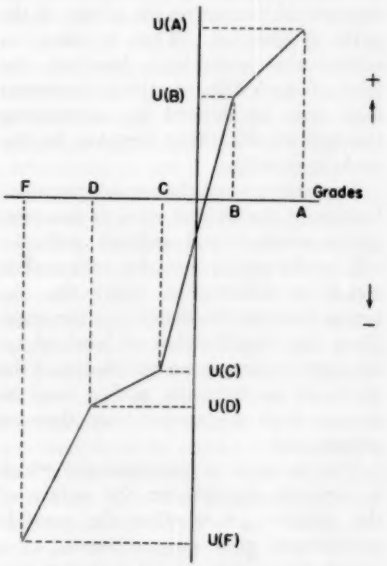

FIG. 3. Graphical representation of the utility of grades for an individual whose level of aspiration is "B."

FIG. 4.

largest distance (difference) in utility between it and the next lower goal.

Figure 3 demonstrates this graphically. The curve represents the utility function of a student who aspired for a B and who made the choices discussed above, among others. It may be seen that the differences between the utility of B, U(B), and the utility C, U(C), is larger than any of the other differences. The ordered metric utility function may also be represented as it is shown in Fig. 4.

LEVEL OF ASPIRATION AS A PROBLEM IN MEASUREMENT

The preceding discussion is directed to the point that under certain conditions the task of ascertaining an individual's level of aspiration reduces to the task of measuring his utility of the goals in question. That is, when an achievement variable is involved, the level of aspiration on the achievement scale may be derived by ascertaining the individual's utility function for the goals in question.

How strong must the measurement be? Obviously the weaker sorts of measurement—nominal and ordinal scaling—will not be satisfactory, for such scaling yields no information about the distances between the entities on the scale. Since the identification of level of aspiration requires some knowledge of the distances on the scale, utility must be measured on a stronger scale than an ordinal one.

The strength of measurement which is required depends on the nature of the variate—on whether the possible achievement goals are represented by a continuous variate or are a discrete set of entities with an underlying continuum.

If the possible goals are on a continuum and if the achievement scale is represented by a continuous variate, i.e., if the scores (goals) may have any value in a certain range, then at least *interval* measurement of utility is necessary to derive level of aspiration. In interval measurement, the distances between points (i.e., the intervals) are known numerically (**22**, **26**, **27**), and thus the numerically largest distance may be identified and the level of aspiration thereby located.

If the possible goals are discontinuous and if the achievement scale consists of a discrete set of entities, i.e., if the scores (goals) can take only isolated values, then at least *ordered metric* measurement is necessary to derive level of aspiration. In strength, an ordered metric scale falls between an ordinal scale and an interval scale. An ordered metric scale (**4**, **5**, **21**) not only gives a ranking of the entities involved but also a ranking of the distances between the entities. With an ordered metric scale we can identify the largest distance and, inasmuch as the individual's choice is limited to the discrete set of goals available, we thereby find that goal which best represents his level of aspiration of those available.

The problem of obtaining an interval measure of utility has been dealt with by Davidson, Siegel, and Suppes (**7**). Their paper includes an axiomatization of the interval measurement of utility as well as a report of an experiment in which interval scales of utility were successfully derived for 15 subjects.

A method for obtaining an ordered metric measure of utility has been developed by Siegel (**21**). Examples of applications of this method in tests of various hypotheses are given in (**15**), (**23**), and (**24**).

Most level-of-aspiration situations, if not all, involve a discrete set of goals. Even if there is continuity underlying

the achievement scale, as is the case in achievement in throwing darts at a board with a bull's-eye, the possible goals are usually broken up into a discrete set. In the case of the dart board, these are concentric circles around the bull's-eye. Thus it would seem that for most situations measurement in an ordered metric scale is all that is necessary to derive an individual's level of aspiration.

To test the validity of the statement that the level of aspiration is that goal which is the least upper bound of the largest distance on an ordered metric scale of utility, a study was conducted which is reported fully elsewhere (1). A summary report of it is included here to lend greater clarity to some of the topics discussed.

EXPERIMENTAL VERIFICATION

Becker and Siegel (1) have conducted a study wherein they tested the hypothesis that the ordered metric utility function would yield information on the individual's level of aspiration.

Subjects were 20 students enrolled in an elementary course in statistics. These were students who volunteered to gamble with the instructor for their midterm grade, with the understanding that the grade they obtained in the gambling session would be entered in the course records in lieu of their score on the regularly scheduled midterm examination.[3] An ordered metric scale of the grades A, B, C, D, and F was derived for each subject by the method described elsewhere (1, 21). This method derives the subject's scale from his choices among alternatives like those exampled above.

[3] After all data had been collected, the nature of the experiment was explained to all participants in individual interviews with them, and all agreed to take the examination and receive a grade based on their performance on it.

The subjects were required to make choices between alternatives in a number of offers. Each of the offers was numbered, and the subjects understood in advance that the offer on which the pay-off (midterm grade) would be based would be selected at random from all of the offers given. Therefore each decision they made had an equal likelihood of being the crucial one on which their midterm grade depended (cf. 21, p. 214; 20, p. 29).

After the subjects had made all the required choices, two ruses were introduced. First, the allegedly random device by which each student's grade would be determined was actually controlled so that each subject "won" a C for his grade. Some subjects responded to this pay-off with considerable dismay and disappointment. In response to this, the experimenters perpetrated the second ruse. The group of subjects was told that if they were dissatisfied with the grade they had won, they could obtain an interview with the instructor and the other experimenter, in which perhaps some way of raising their grade could be worked out. The implication contained in this announcement was that performance of extra work would be the mode of raising the grade. The subjects were told that any who desired such an interview would have to wait until the instructor placed an urgent long-distance telephone call, after which he and the other experimenter would return to conduct individual interviews with those students who desired them. The instructor said he would return in about five minutes, but actually his absence was unexplainedly extended to an hour's time before he returned for individual interviews. As will become clear, this ruse permitted a test of one of the hypotheses of the study.

The interviews were structured to obtain a careful independent measure of each subject's level of aspiration for

his midterm grade. Both experimenters were present, and their independent judgments of each subject's level of aspiration were in very close agreement ($r_s = .99$). The interviews were conducted before either experimenter had any information concerning any subject's ordered metric scale of utility for grades.

The experiment was designed to test two hypotheses. For this summary it may suffice to state these briefly and to report the outcome of each hypothesis test.

Hypothesis I was that those subjects who would not wait for an interview with the instructor would be persons on whose ordered metric scales the largest distance was between D and F—persons whose ordered metric scales would reveal that their level of aspiration was below the C which they had won. This hypothesis was supported by the data. Four subjects left the room before the experimenters returned from their hour-long errand. All had ordered metric scales with the largest distance between D and F. Seemingly they were satisfied with the C grade they had been assured and therefore saw no reason to seek an interview.

Hypothesis II was that subjects' levels of aspiration, as determined from their ordered metric scales of utility, would be positively correlated with their levels of aspiration as judged from the interview material. This hypothesis was confirmed: the correlation between the two independent indices of level of aspiration was $r_s = .83$.

GOALS FOR FUTURE RESEARCH

Child and Whiting (3, p. 508) have formulated five general statements which contain the conclusions which may reasonably be drawn from research on level of aspiration to date:

1. Success generally leads to a raising of the level of aspiration, and failure to a lowering.
2. The stronger the success, the greater is the probability of a rise in level of aspiration; the stronger the failure, the greater is the probability of a lowering.
3. Shifts in level of aspiration are in part a function of changes in the subject's confidence in his ability to attain goals.
4. Failure is more likely than success to lead to withdrawal in the form of avoiding of setting a level of aspiration.
5. Effects of failure on level of aspiration are more varied than those of success.

Each of these generalizations may be treated as hypotheses to be tested in experimentation employing the decision-theory approach outlined here. If such experimentation leads to confirmation of the hypotheses, this would strengthen confidence in the validity of that approach.

Simon (25, p. 110) points out that in most models of rational choice it is assumed that the individual will evaluate the outcomes of all possible alternatives before he makes any choice. In realistic human situations requiring decision, however, alternatives often must be examined sequentially and decisions must be made as the alternatives are presented. When this is the case, and when the individual does not know what variables determine the order of the offers (and therefore does not know whether the future alternatives are likely to be "better" or "worse" than those with which he has already been confronted), he may well choose the first satisfactory alternative which is offered. In the terms of the present paper, the individual may choose the first alternative containing an outcome at or above his level of aspiration. The techniques suggested in this paper could be used to provide an empirical test of this hypothesis.

Simon offers another interesting hypothesis which might be tested in the context of the present approach. As the individual is being presented with a sequence of choice situations, he may find

that it is easy for him to discover satisfactory alternatives among them. If so, his level of aspiration will rise. On the other hand, he may find it difficult to discover satisfactory alternatives among those made available to him, and in this case his level of aspiration will fall. Simon suggests that such changes in level of aspiration would tend to guarantee the existence of satisfactory solutions to the decision situation, for failure to discover initially satisfying alternatives would depress the level of aspiration and thereby bring satisfactory solutions into existence.

SUMMARY AND CONCLUSIONS

This paper suggests that Lewinian theory concerning level of aspiration may be integrated with certain parts of decision theory. An achievement scale may be viewed as a scale of utility of the achievement goals. A formal definition of level of aspiration in terms of utility is offered. The problem of ascertaining a person's level of aspiration reduces to the problem of measuring his utility of the achievement goals. It is hypothesized that level of aspiration is associated with the largest distance on an individual's utility scale. If this is so, with a discrete set of goals, ordered metric measurement is sufficient for identifying a person's level of aspiration, since an ordered metric scale contains not only a ranking of the entities (achievement goals) but also a ranking of the distances between them. Certain experimental evidence which supports the hypothesis is summarized. Suggestions for future research are presented; these draw upon the ideas presented here, together with those of other workers in the fields of level of aspiration and decision theory.

In conclusion, it would seem that a useful behavioral model of decision making should include not only the concepts of utility and subjective probability, as do the present models, but should also include a formulation of the effects of level of aspiration and reinforcement on utility. That is, the model should include recognition that utility has a model in its own right, in which the main concepts are level of aspiration (LA) and reinforcement effects (R).

In terms of such an extended model, it may be said that if various alternatives are available to an individual, he will choose from among these alternatives, toward each of which he has a subjective probability of attainment and a utility, so as to maximize subjectively expected utility, SEU. That is, the individual will choose so as to maximize

$$SEU = \sum_i p_i u_i,$$

where

$$u = f(LA, R).$$

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A NOTE ON THE CRESPI EFFECT

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The classical Crespi (3) and the Tolman and Honzik (6) studies report abrupt shifts in performance following a discrete shift in the magnitude of the reward. The writer recently attempted to reconcile their data with his own where this "abruptness" effect failed to occur (4). This led to certain inferences about the role of exploratory activity in performance measures. In addition, Hull's habit construct was revised to improve its handling of the learning process following such a shift in reward magnitude. The present paper attempts to extend that study specifically to include Crespi's contrast effect.

It was argued in that paper that the early trials in most learning experiments involve much exploratory behavior which competes, performance-wise, with the one response measure that concerns *E*, that based on the acquisition of a goal expectancy. Learning about the maze or runway takes time, however, and knowledge about the goal forms only a small portion of the total learning. The poor performance in the early trials is a measure of this large amount of learning going on as it is reflected in exploratory behavior. Thus, even the runway does not give us a single response tendency (2). But as trials continue, exploration drops out and the competing goal response becomes more and more dominant: performance is now approaching an asymptote.

At this point in the Crespi experiment the reward magnitude was changed. All that his rats now had to learn was that a new magnitude was present; runway exploration, reflected in gradual improvement, was essentially completed during the initial learning. The response change was therefore abrupt relative to the original rate of response improvement.

Now consider the contrast effect. The level of runway familiarity or learning at the time of the reward-shift will be a function of the previous amount of runway exploration, and this in turn will be partially determined by the magnitude of the initial incentive. A large reward will compete more effectively with the exploratory tendency than a small one. This is inferred from the greater running speeds found under large rewards. Thus a large-reward group knows *less* about the runway than a small-reward group (cf. [1]). But unfamiliarity is a condition that arouses the exploratory tendency. If the large-reward group is now put on a smaller incentive they will not only begin "extinguishing" to some extent but they will also begin exploring—and to a greater degree than would a group on this small incentive throughout training, simply because they know less about this runway than would such a control group. The same reasoning should lead to the "elation" effect for the upward-shift group. An initially small reward means a large amount of initial exploration. Under a new and larger amount, they will need to explore less than would a group under that larger amount throughout training; they can therefore run faster than this group.

But the upward contrast effect does not always appear. Negative results were obtained in Spence's laboratory in several experiments designed along the lines of the Crespi study (5, pp. 127–

[1] The writer is indebted to Dr. Keith J. Hayes for his critical reading of this paper.

133). Spence concludes that the "elation" effect is due to a failure to train animals to their asymptotic speeds, additional trials being the source of the effect rather than the incentive shift. However, it may not be the asymptote per se. If the asymptotic speed is too close to some relative upper limit set by the given experimental situation, drive level, and quality of reward, there simply will not be enough room for the effect to show. To obtain the contrast effect then, the large reward must not be too large. Logically, of course, the same reasoning should apply to the downward effect. If the small reward leads to speeds little better than the operant speed, we would also fail to observe any effect.

A preliminary test of the present interpretation will introduce one condition into the Crespi study that was apparently omitted: Thoroughly familiarize the animals with the runway prior to Trial 1. This should lead to greater rates of response improvement under the initial incentives, and so tend to eliminate the relative abruptness of performance changes following the incentive shifts. In addition, the asymptotes of the original performance curves should be greater than those obtained by groups lacking preliminary exploration: An upward contrast effect would already be present. Whether positive and negative contrast effects will appear following the incentive shifts, however, depends upon (*a*) the amount of preliminary exploration and (*b*) how long such groups are maintained under their initial amounts of reward. If no differences in runway familiarity are present at the time of such incentive shifts, then no contrast effects should follow.[2]

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[2] It might be noted here that Crespi's use of a particularly long runway (20 ft.) upon which speed was recorded, rather than just latency, no doubt favored the exploratory tendency and thus gave him clear-cut results (cf. [7]).

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